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ART. XXVII.—*On the Number and Distribution of the Bright Fixed Stars;* by B. A. GOULD.

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THE magnificent work of Argelander entitled "Durchmusterung des nordlichen Himmels," is well known to lovers of astronomy. His problem was no less than the formation of a complete list of all the stars of the northern hemisphere to the ninth magnitude inclusive, together with as many as possible of the 9–10 magnitude. The undertaking was successfully carried out, affording not only an exhaustive series of charts, but likewise a "working-list," which an association of northern observatories is now employing for the determination of the accurate positions of the 315,000 stars which it contains. It furthermore records the aspect of the visible heavens, at the time, with an accuracy amply sufficient for all purposes which do not require minute precision.

In this work, the magnitude of each star was estimated to the nearest half unit as it passed through the field of view; and since all the stars were observed more than once, and most of them several times, the mean of the several estimates was taken and is given in the published catalogue to the nearest tenth of a unit. In 1869 Professor Littrow of Vienna made a careful enumeration of the number thus given for each degree of magnitude, in order to ascertain how far the results would indicate an approximate uniformity of distribution for the stars lying within the portion of space under consideration.

Could we assume, 1st, that, in general, the apparent brightness of a star depends upon its distance from us, all being of the same order of intrinsic brilliancy, and 2dly, that stars are distributed through space with approximate uniformity, the total number visible, down to any given magnitude, would be proportional to the contents of the sphere within which stars of this magnitude are contained. Thus the degree of approximation to the truth, which is fairly attributable to the hypothesis mentioned, may be inferred from the degree of accordance which can be obtained between two series of numbers, the one representing the number of stars of each successive order of magnitude actually existing in the heavens, and the other being proportional to the contents of those spherical shells whose limiting radii correspond to these respective magnitudes. If any accordance at all satisfactory should be found to exist, it would render the assumptions probable to a corresponding extent, and would afford at the same time an independent, even though indirect and crude, determination of the constant ratio between the amounts of light which belong to two successive magnitudes, and thereby an indication of the relative distances of the stars.

If the data on which the inquiry is based be sufficiently extensive, we may reasonably expect that the effects of considerable deviations from perfect exactness in the general hypotheses will be masked in the totals, provided the assumptions be not essentially erroneous. But there are serious difficulties in the way of an accurate comparison, arising from the inadequacy of our data, in consequence of inevitable errors in the estimates of magnitude, the moral impossibility of maintaining exactly the same ratio for different degrees of brightness, and other almost equally important obstacles. Notwithstanding all these, Professor Littrow considers his results as justifying the belief that there is a considerable degree of general uniformity, both in the distribution and the intrinsic brilliancy of the fixed stars which belong within the limits of this investigation; sufficient, indeed, to warrant an application of the formulas, resulting from this inquiry, to regions outside of the distance of 9th magnitude stars. From a discussion of the numbers given by the Durchmusterung as far as the 8th magnitude, assorted according to whole units, he obtains the value 0·423 as the ratio of brilliancy between typical stars of two successive magnitudes; and similarly from an assortment by half magnitudes, comprising all stars to the 8th magnitude inclusive, he obtains the value 0·431. Each of these computations gives, for the average distance of a star of the 9th magnitude, about 26 times that of the most distant first magnitude star.

The discordances between the numbers obtained by enumeration from the Durchmusterung and the empirical values deduced from Professor Littrow's formula, vary satisfactorily in sign; and the sums of the positive and of the negative residuals are nearly equal for the groups from $3\frac{1}{2}$ to $8\frac{1}{2}$ inclusive, when the residuals are reduced to fractions of the computed values. Still some of the discordances are large, the difference for stars of the 4th magnitude amounting to more than one-third part of the empirical number, while for those of the 9th magnitude it would exceed one-half of the whole. The appended table exhibits the number of stars for each half-unit of magnitude as obtained from the counting, and from Littrow's formula, according to which the number comprised within the half-magnitude m is given by the expression

$$Z_m = 0{\cdot}6098\ (3{\cdot}5295)^m.$$

To these I add the values corresponding to another formula, which I have deduced for the purpose of giving a somewhat better representation to the stars brighter than the $3\frac{1}{2}$ magnitude. In this the light-ratio for each magnitude is 0·4523 and the number of stars comprised in the half-magnitude m is

$$Z_m = 1{\cdot}0691\ (3\ 2878)^m.$$

The residuals appended to each of the empirical series represent the excess of the computed numbers over the observed ones, expressed in hundredths of the former.

STARS OF THE NORTHERN HEMISPHERE.

According to the Durchmusterung.

Mag.	Counted.	Littrow's formula.	Difference.	New formula.	Difference.
1	6	3		$5\frac{1}{2}$	−11
$1\frac{1}{2}$	4	4		$6\frac{1}{2}$	+38
2	22	8		12	−90
$2\frac{1}{2}$	12	14	+14	21	+43
3	51	27	−89	38	−34
$3\frac{1}{2}$	60	50	−20	69	+13
4	128	95	−35	125	+ 2
$4\frac{1}{2}$	140	178	+21	232	+40
5	379	334	−13	411	+ 8
$5\frac{1}{2}$	463	628	+26	745	+37
6	1242	1179	− 5	1350	− 8
$6\frac{1}{2}$	2231	2215	− 1	2449	+ 9
7	4608	4161	−11	4441	− 3
$7\frac{1}{2}$	6878	7817	+12	8052	+14
8	14525	14686	+ 1	14600	+ 1
$8\frac{1}{2}$	28486	27590	− 2	26474	− 8
9	78185	51830		48003	

The marked defect of the computed numbers for stars of the 9th magnitude has led me to include the several values for this magnitude in the table, although they were intentionally omitted in deducing the formulas. I do not think that any

expressions of the form ab^m can be found which will represent all the values to the $8\frac{1}{2}$ magnitude inclusive, with an essentially better accordance. The numerical values of the residuals might be very slightly improved by using the method of least squares, but not to any important extent. The discordance of the formulas given would be still greater for stars of the $9\frac{1}{2}$ magnitude, since Littrow's gives 97,370 stars and my own only 87,040, while in the Durchmusterung there are 177,505 stars of the 9·3, 9·4 and 9·5 magnitudes; those of the 9·6 and 9·7, which are needed in addition to make up the half-magnitude group, not having been observed.

It would thus appear that, unless the fainter stars of the Durchmusterung have been estimated much too bright, the hypotheses in question cannot be extended beyond the $8\frac{1}{2}$ magnitude, even if they are to be regarded as approximately representing the tenth for the stars up to this limit, the distance corresponding to which would be, according to Littrow's formula, about 22 times, and according to the other formula about 18 times, that of a star of the first magnitude.

The Uranometria Nova of Argelander, which gives the carefully observed magnitudes of all the stars which he could distinguish with the naked eye, afforded the standard for the magnitudes of the Durchmusterung; yet the first glance makes manifest large discrepancies between the rapidly made estimates in the latter work and the sharp determinations of the former. The recently published revision and extension of the Uranometria by Professor Heis of Aachen, assigns the magnitude to the nearest third of a unit for every star which he could discern, the lowest being $6\frac{1}{3}$, or one-third of a magnitude fainter than Argelander's inferior limit. The far greater precision of these determinations would give a more trustworthy basis for our inquiries than the Durchmusterung affords, were the numbers large enough to eliminate such irregularities as may justly be treated as accidental; but this seems not to be the case.

The completion of our Argentine Uranometry now augments the number of accurately determined stars, and renders it possible to assign the actual magnitudes for all stars throughout the heavens which are easily visible to the naked eye. I have taken much pains to secure an accordance between the adopted scale of magnitudes and that employed by Argelander in his Uranometry, and regard it as unlikely that the probable error of our individual magnitudes exceeds one-tenth of a unit. So too does Heis seem to have omitted no efforts for securing an accordance of his work with the same standard, and it is improbable that any essential error can exist in these estimates, made as they are by an astronomer of exceptional keenness of sight.

It is thus easy to ascertain the total number of stars in the firmament for each grade of magnitude within Heis's limits. The results are especially trustworthy, since every individual magnitude has been determined by careful and repeated comparisons with established standards and the same scale; and there now arises the interesting question, how far the accurate numbers given by the two Uranometries, for stars as bright as the 6th magnitude throughout the entire heavens, would agree with the rougher estimates of a number of stars eleven times greater, but situated in the northern hemisphere only and including the $8\frac{1}{2}$ magnitude?

I have therefore devoted some labor to the independent determination of a formula which should represent as well as possible the results derived from the Uranometries alone, upon the hypothesis already stated. The best value which I have been able to deduce for the constant ratio of the light of stars of successive magnitudes is 0·4988, the degree of accordance of which with observation upon our fundamental assumptions may be inferred from the appended table, which gives the number of stars for each half unit of magnitude, as deduced from the Uranometries, and from the formula

$$Z_m = 3{\cdot}2384\ (3{\cdot}0184)^m,$$

together with the residuals, determined as in the preceding table. Those stars of Professor Heis's Uranometry which are given for the fractional thirds of a magnitude, are combined to form the numbers for the fractional halves in our table; a crude procedure, but the only practical one under the circumstances.

NUMBER OF STARS IN THE HEAVENS.

Mag.	Uranometries.			Formula.	Difference.
	N.	S.	Total.		
1	8	6	14	$15\frac{1}{2}$	+10
$1\frac{1}{2}$	7	4	11	17	+35
2	25	20	45	395	−53
$2\frac{1}{2}$	35	33	68	51	−33
3	55	41	96	89	− 8
$3\frac{1}{2}$	103	87	190	155	−23
4	132	108	240	269	+18
$4\frac{1}{2}$	254	154	408	467	+17
5	392	240	632	811	+22
$5\frac{1}{2}$	696	563	1259	1409	+16
6	1374	1075	2449	2449	0
$6\frac{1}{2}$	----	2022	----	4255	----
7	----	3317	----	7392	----

Various inferences are suggested by this table. The formula corresponding to our hypotheses differs greatly from the former ones, both in the coefficient and the ratio. The degree of

accordance is not such as to warrant any great faith in the correctness of our assumptions, yet a certain approximation in the numbers cannot be denied, extending apparently as far as the 8th magnitude, if we assume the numbers of the Durchmusterung, augmented proportionally for application to the entire firmament. For the higher magnitudes the formula gives numbers even more glaringly in defect than those derived from the Durchmusterung. And finally a comparison of the numbers in the northern hemisphere, taken from Heis, and those of the southern hemisphere, derived from the Uranometria Argentina, shows a decided excess of stars in the northern sky, a fact which is itself at variance with our assumption of an approximately equable distribution.

Yet although the accordance of the observed magnitudes with any series of numbers in geometrical progress is far from being close enough to warrant any important inferences regarding the total number of stars of any magnitudes outside of the limits of observation, it affords the only means at our disposal for any of those rough estimates which are needed in cosmological inquiries regarding these numbers. As soon as we have extended our researches to that distance at which the agglomeration of stars in the Milky Way begins to be appeciable, all further inquiries of this character are futile, and it becomes necessary to consider the Galaxy by itself. From a comparison of the geometric series with the numbers observed for stars not beyond the $8\frac{1}{2}$ magnitude, Littrow has inferred that the same formula may be applied to regions considerably beyond this limit, and from the consideration of W. Herschel's star-gauges in the poorest regions of the sky, he fixes upon a magnitude not far from $11\frac{1}{2}$ as indicating the outer limit of this equable distribution, and thus assigns $4\frac{1}{2}$ millions as the probable number of stars within this limit. To me neither premise appears very tenable. If the influence of the Milky Way is not appreciable even for stars of the 9th magnitude, then the number of stars at a less distance than the limit of galactic agglomeration is not even approximately conformable to geometric progression; so that these inferences from the star-gauges must be illusory. The rapid increase in the number of stars after passing the 9th magnitude may be partially accounted for by the difficulty of estimating magnitudes correctly, near the inferior limit of brightness; yet very large allowances for over-estimation of the fainter magnitudes fail to permit any satisfactory representation by a geometric progression. It is impossible to represent even approximately the more than 150,000 stars of the 9th magnitude, together with the numbers for magnitudes below $4\frac{1}{2}$, by any such formula, without over-representing the intermediate numbers by more than one-half. And even then,

we fail to obtain an adequate value for the stars beyond 9 and 9½. We may reproduce the numbers which the Uranometries give for the 6th magnitude and the Durchmusterung for the 9th, by means of a series which doubles the number of stars for each successive half-magnitude; yet even this, when extended to stars of the 9½ magnitude, would assign to the entire heavens a less number than the Durchmusterung gives for the northern hemisphere alone. I attribute, however, to this last consideration comparatively little weight, for the reason already indicated.

In the second column of the annexed table, the number of northern stars corresponding to each half-magnitude is reproduced, being taken from Heis's Uranometry for magnitudes up to the 6th inclusive, and from the Durchmusterung for higher ones. The last line, however, is not for the full half-magnitude corresponding to 9½, but merely for 9·3, 9·4 and 9·5; the remaining two, 9·6 and 9·7, not being contained in Argelander's work. The third column contains the numbers from the Uranometria Argentina, and the fourth the corresponding ones for the entire heavens, being the sum of the two preceding as far as the 7th magnitude, and thereafter the double of the numbers observed in the northern hemisphere. The final column shows, for the sake of comparison, the values which result from the simple hypothesis that the number of stars doubles for each successive half-magnitude applied to the observed number of the 6th magnitude. Simple as it is, this series presents less violent discordances than any of the others which I have deduced, for magnitudes above the 4th. For stars

TOTAL NUMBER OF STARS.

Mag.	N.	S.	Total.	Formula.
1	8	6	14	4
1½	7	4	11	5
2	35	20	45	10
2½	25	33	68	19
3	55	41	96	38
3½	103	87	190	76
4	132	108	240	153
4½	254	154	408	306
5	392	240	632	612
5½	696	563	1259	1224
6	1374	1075	2449	2449
6½	2231	2022	4253	4898
7	4608	3317	7925	9796
7½	6878	----	13756	19592
8	14525	----	29050	39184
8½	28486	----	56972	78368
9	78185	----	156370	156736
	177505	----	355010	161660

brighter than these, the discordances, although relatively enormous, are intrinsically small. The total number of all stars to the 4th magnitude inclusive is 664 according to direct observation, but would be only 305 by the empirical series. And the theory of an approximately equable distribution for stars as bright as the 9th inclusive thus appears less improbable, if we may suppose some 360 additional stars to be situated in our immediate vicinity.

In this connection I desire to mention a fact which early attracted and repeatedly compelled my attention during my residence in South America. It has generally been assumed that the number of visible stars of any given magnitude,—whether brighter or fainter,—diminishes as their distance from the Milky Way increases. In the elevated position and pure atmosphere of Cordoba, this nebulous circle is seen with a vividness far surpassing that to which we are accustomed here, and moreover most of that portion which lies in the southern hemisphere is intrinsically brighter than the northern half; so that its position is far more clearly defined than I have ever seen it elsewhere. And few celestial phenomena are more palpable there than the existence of a stream or belt of bright stars, including *Canopus*, *Sirius* and *Aldebaran*, together with the most brilliant ones in *Carina*, *Puppis*, *Columba*, *Canis Major*, *Orion*, &c., and skirting the Milky Way on its preceding side. When the opposite half of the Galaxy came into view, it was almost equally manifest that the same is true there also, the bright stars likewise fringing it on the preceding side, and forming a stream which, diverging from the Milky Way at the stars *Alpha* and *Beta Centauri*, comprises the constellation *Lupus* and a great part of *Scorpio*, and extends onward through *Ophiuchus* toward *Lyra*. Thus a great circle or zone of bright stars seems to gird the sky, intersecting with the Milky Way at the Southern Cross, and manifest at all seasons, although far more conspicuous upon the Orion side than on the other. Upon my return to the North, I sought immediately for the northern place of intersection; and although the phenomenon is by far less clearly perceptible in this hemisphere, I found no difficulty in recognizing the node in the constellation *Cassiopea*, which is diametrically opposite to *Crux*. Indeed it is easy to fix the right ascension of the northern node at about 0h. 50m., and that of the southern one at 12h. 50m.; the declination being in each case about 60°, so that these nodes are very close to the points at which the Milky Way approaches most nearly to the poles. The inclination of this stream to the Milky Way is about 25°, the Pleiades occupying a position midway between the nodes.

A considerable portion of the bright stars of our firmament is situated within this zone or stream, or in its immediate vicinity.

It has been a source of surprise to me that it had not previously attracted the notice of astronomers, and since writing the foregoing paragraphs I had begun the preparation of some data in statistical form to demonstrate its existence, when I discovered that it had been alluded to by Sir J. Herschel in his Results of Observations at the Cape of Good Hope. His words are as follows (p. 385):

"It is .... in the interval between *η Argus* and *α Crucis* that the Galactic circle, or medial line of the Milky Way, may be considered as crossed by that of the zone of large stars which is marked out by the brilliant constellation of *Orion*, the bright stars of *Canis Major* and almost all the more conspicuous stars of *Argo*, the *Cross*, the *Centaur*, *Lupus* and *Scorpio*. A great circle passing through *ε Orionis* and *α Crucis* will mark out the axis of the zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbors in the sidereal system (if really such) form part of a subordinate sheet or stratum, deviating to that extent from the general mass which seen projected on the heavens forms the Milky Way."

Yet he does not appear to have recognized the fact that this zone of bright stars may be traced with tolerable distinctness through the entire circuit of the heavens, forming a great circle as well defined as that of the galaxy itself.

These stars, or, more strictly speaking, this excess of stars, in the regions in question, must be deducted from our total before the remainder of the fixed stars can legitimately be subjected to statistical discussion with a view to determining the law of their distribution, and the distance at which the agglomeration which we recognize in the Milky Way begins to make itself manifest. Then we shall perceive that the inferences from the accurate estimates in the Uranometries and the rough ones of the Durchmusterung are by no means discordant, and that the distribution of the fixed stars, up to the 9th magnitude inclusive, is not merely tolerably uniform but approximately such that the number of stars doubles for each successive half-magnitude. The distance corresponding to the 9th magnitude is from thirty-two to forty times that of the faintest first-magnitude star; and the light-ratio between stars differing by a single magnitude becomes 0·3968. This is very close to the ratio 0·4, which photometric researches have seemed to indicate as best expressing the actually existing scale, and which is the value usually accepted. Did we adopt precisely this ratio, we should find 315 as the total number of stars as bright as the 4th magnitude, being only ten more than was given by the value just considered, while the computed numbers for all other magnitudes below the ninth would be brought somewhat nearer

to the observed ones, and for the 9th magnitude itself would be 151,260.

The phenomena and numerical relations to which I have rererred in this paper seem of considerable importance in their bearing upon the position of our sun in its cluster, the form of that cluster, and the scale of distances between its constituent stars.

ART. XXVIII.—*The Deportment of Titanium with reagents in Iron Ores containing Phosphoric Acid;* by E. H. BOGARDUS.

(Read before the Natural History Society of Rutger's College, New Brunswick, N. J., April 9, 1874.)

THE injurious action of titanium upon iron when present in the metal in considerable quantity, and its frequent occurrence in nature, render it important that a reliable method for its detection be known, and that its presence or absence be proved in all analyses of ores.

During the summer of 1872, while engaged in the examination of some samples obtained in the mines of New Jersey, I encountered a difficulty, which I shall explain somewhat in detail. Subsequent experiments proved the presence of titanium, and that this substance had caused me my trouble. The deportment of the element was so different from what is taught in books of analyses, and from what I had previously observed, that it seemed to me that the usual directions given for its detection were faulty and liable to mislead the analyst. A more extended investigation served to strengthen this impression, and, in view of the importance of the subject, led to the preparation of the following paper.

Although it was not intended at that time to make complete analyses of the ores, it was thought important to test for titanium. To this end a sample was fused with bisulphate of potash and digested in cold water. The dissolved substance was then filtered and the clear liquid treated with sulphuretted hydrogen to effect the reduction of the iron. This should have caused no precipitate of titanium under ordinary circumstances. Sulphur, of course, separated in proportion to the iron reduced, and was removed by filtration. The liquid was now boiled, and as it remained clear, even after long heating, the absence of titanium might have been regarded as demonstrated. The examination, however, did not stop here: it was decided to test the sulphur remaining on the filter. On burning this precipitate a residue was obtained amounting to nearly six centigrams, although but a gram of ore had been

taken for the examination. As sulphur would have been vaporized by the heat, the burning, of course, proved the presence of some other body. As the absence of zinc and the members of the fifth and sixth groups had been proved, the only elements supposed to be precipitable by sulphuretted hydrogen under the existing conditions, the supposition was that a mistake had occurred. The experiment was, therefore, repeated and with identical results. As it now seemed certain that sulphuretted hydrogen had caused a precipitate, the next step made was to determine its character. The substance was white both before and after heating, and resembled closely sulphide of zinc. After burning, it held no sulphur. The absence of color and sulphur would alone have seemed sufficient proof of the absence of the fifth and sixth groups, without the concurring evidence as afforded by the previous regular qualitative tests. But since it could not be denied that a substance had formed and its formation seemed an anomaly, it was determined to examine again and this time for all bases without exception. It would require too much time, and would serve no useful purpose, to enter into a minute description of the course taken in this examination, so I will confine myself to a statement of a few of the properties as developed during the experiments. After burning the precipitate and fusing it in bisulphate of potash, it was insoluble in water. It was sparingly soluble in hydrochloric acid immediately after precipitation with sulphuretted hydrogen, but it could be dissolved after repeated boiling in fresh quantities of acid. Fusion in carbonates of the alkalies rendered it soluble in hydrochloric acid It was insoluble or nearly so in boiling solution of potash; precipitated with difficulty or not at all in slightly acid solutions. It colored microcosmic salt brown, gave a transient green color to the blowpipe flame, fused to a black non-magnetic globule on heating on charcoal, and when dissolved in hydrochloric acid imparted a brown tint to turmeric paper like that produced by boracic acid. The effect on turmeric paper led to the supposition that the substance was partly zirconia. The thought at length suggested itself that as there was no sulphur in the precipitate, its formation might be due to a reducing action. Sulphurous acid being a powerful reducing agent, it was determined to substitute this for sulphuretted hydrogen. A sample of ore was fused as at first in bisulphate of potash; then dissolved, filtered, and the reducing gas passed through the liquid. A red coloration at first appeared, followed by a white precipitate like that obtained in the first instance. A quantity of ore was now digested in hydrochloric acid and filtered to separate the insoluble matter. After nearly neutralizing the free acid with ammonia, acetate of ammonia was

added in excess, and then acetic acid in large quantity. On passing sulphuretted hydrogen a white precipitate was again obtained. This experiment was repeated several times with identical results, proving that the substance would form in a solution holding free acetic acid. The precipitate was again subjected to a qualitative examination and this time with success. Previously no search had been made for acids: it was now decided to test for them. Phosphoric acid was soon found and the true character of the substance was easily determined. The precipitate was fused in carbonates of soda and potash and digested in water. The phosphoric acid was all dissolved and was detected with molybdate of ammonia. The nearly white residue was proved to be a mixture of titanium and iron. One-half gram of the ore was mixed with an equal weight of an ore holding a large quantity of titanium. The mixture was fluxed in bisulphate of potash, and finally subjected to the action of sulphuretted hydrogen. A precipitate weighing six centigrams was obtained. The sample first examined could give only two and a half centigrams of precipitate. This experiment was followed by the regular tests for titanium, and its presence was soon demonstrated. Rutile was now powdered and mixed with phosphate of soda and an ore free from titanium. After fusion with bisulphate of potash and digestion in water, sulphuretted hydrogen gave a precipitate as in the previous trials. As it now seemed proved that sulphuretted hydrogen could precipitate titanium, it became interesting to know whether the precipitate formed was definite in its composition. It was intended to pursue this question to a satisfactory solution if possible, but owing to the pressure of other matters but little was done: too little to justify the assertion that the precipitate was a definite compound. Several experiments were subsequently made, on mixed samples of rutile and an ore and phosphate of soda, and a precipitate was obtained in every instance with sulphuretted hydrogen. In one case the phosphate of soda was dissolved in water and added to the fused rutile and ore. Rutile was now fused in bisulphate of potash and digested in water; after filtering, sulphuretted hydrogen gave no precipitate. This trial was made to determine whether iron was necessary to precipitation: the result seemed to demonstrate the necessity of its presence and that the formation of a precipitate is dependent upon the reduction of iron. The deportment of titanium with phosphoric acid led to the thought that possibly it might be employed as an agent in the quantitative separation of the phosphorus in iron ores. No experiments were made with a view to the settlement of this question. In all the co-precipitations of titanium and phosphoric acid, the latter was present in

excess. It is possible that titanium in excess would have prevented precipitation with sulphuretted hydrogen. The subject seems worthy of an investigation. Let us now pass in brief review over the ground we have traversed, and endeavor to see what light has been thrown upon the properties of titanium and whether the method of its detection, by fusion with bisulphate of potash, as given in books, is all that could be desired, and whether it could not occasionally mislead the analyst. First, the properties of titanium. It was found that it would not precipitate readily in an acid solution on boiling. Phosphorus seemed to hinder, if not to entirely prevent, precipitation; so this method, which is much employed, is faulty. Indeed, this was known before, for some works of analyses tell us that zirconia at least will prevent the separation of titanium from boiling liquids. 2d. We find that titanium colors turmeric paper a brown, or nearly orange color, a fact before known but not generally mentioned in analytical works. 3d. Sulphuretted hydrogen throws down titanium, although no books seem to teach it. Let us now imagine the case of a student testing ores for titanium. He fuses his sample in bisulphate of potash and digests it in water; he filters, and passes sulphuretted hydrogen through the liquid. Sulphur of course separates and gives the solution a milky appearance; he regards this sulphur free from titanium. As the milkiness would render it impossible to see whether a precipitate formed on boiling, he decides to filter and thus render the liquid clear. On boiling no precipitate forms. The titanium has been thrown away with the sulphur, and the analyst has formed a false idea of the composition of his sample. It is plain, then, in case of filtration, after passing sulphuretted hydrogen, that the sulphur should be examined, provided titanium be not found on boiling the liquid.

ART. XXIX.—*Contributions from the Sheffield Laboratory of Yale College.* No. XXXI.—*Experiments on the Decay of Nitrogenous Organic Substances;* by H. P. ARMSBY, Ph.B.

IT has been shown by the investigations of Mulder (Chemistry of Animal and Vegetable Physiology, p. 673), Boussingault (Agronomie, &c., t. i, p. 318), and Dehérain (Annales des Sciences Naturelles, Botanique, t. xviii, p. 147), that the soil and other organic or partly organic substances are capable, under some circumstances, of causing free nitrogen to enter into combination in them, and thus of increasing their content of that element. This increase has generally been attributed to the oxidation of free nitrogen to nitrous or nitric acids.

On the other hand, it has been shown by Reiset (Recherches, &c., sur l'Agronomie, 1865, p. 48), Lawes, Gilbert, and Pugh (Phil. Trans., 1861, ii, p. 501), and Schloesing (Compt. Rend., t. lxxvii, p. 353), that during the decay of nitrogenous organic substances in presence of free oxygen, nitrogen may be evolved in large quantities in the free state. König & Kiesow (Centralblatt f. Agriculturchem, II. Jahrg., 7, 9) have shown that the presence of gypsum prevents this loss of nitrogen.

The following experiments were undertaken in the hope of obtaining some information in regard to the conditions favoring these reactions. The method adopted was to allow organic matter containing a known amount of nitrogen to decay under such conditions that all the ammonia given off could be collected and estimated, and at the end of the experiment to determine again the nitrogen. The experiments were conducted in glass bottles, each of which was closed by a rubber stopper and connected with a three-bulbed U-tube containing standard sulphuric acid to prevent any escape of nitrogen as ammonia, and was also provided with a glass tube for the admission of the gas used in the experiment. Two sets of experiments (marked I. and II.) of four each were made. In I. the atmosphere in the bottles consisted of air purified from ammonia and nitric acid, in II. of nitrogen prepared from air by means of phosphorus and carefully purified. It still contained traces of oxygen.

With the exception of the atmosphere, the conditions of corresponding experiments in the two sets were the same, as far as possible.

The organic matter used consisted of dried and sifted barnyard manure intimately mixed with about one-fourth its weight of dried and pulverized flesh. The mixture contained 2·11 per cent of water. The following were the materials and quantities used:

	Org. matter.	Gypsum.	Potash (KOH)	Water.	Total Nitrogen.
I. 1.	15 grms.	----		6 c.c.	0·486 grms.
I. 2.	15 "	----	0·798 grms.		0·486 "
I. 3.	15 "	15 grms.		6 c.c.	0·486 "
I. 4.	15 "	15 "	0·798 grms.		0·486 "
II. 1.	15 "	----		6 c.c.	0·453 "
II. 2.	15 "	----	0·798 grms.		0·453 "
II. 3.	15 "	15 grms.		6 c.c.	0·453 "
II. 4.	15 "	15 "	0·798 grms.		0·453 "

The potash was from a standard solution, of which 7 c.c. were used in each experiment, equivalent to about 6 c.c. of water. The mixtures were made moist but not coherent. All the inorganic materials used were free from combined nitrogen.

The first set (I.) was begun March 9th, the second (II.) March 31st, the per cent of nitrogen in the organic matter having de-

creased slightly in the interval. Purified air, amounting in all to about 5,800 c.c. for each bottle, was aspirated daily through I. The temperature during the experiments ranged from 50° —80° F., averaging about 70°. The apparatus stood in diffused daylight but not in direct sunlight. The experiments terminated May 15–21st, when the mixtures were dried with suitable precautions, and analyzed for nitrogen and ammonia. The nitrogen was determined by combustion with soda-lime and titration, the ammonia by boiling with magnesia and titration of the distillate. No ammonia was found in the standard acid in the U-tubes. The following are the results calculated on the whole quantity used:

	Materials.	Gain (+) or loss (−) of nitrogen *		G'in of ammonia expressed as nitrogen.
		Weight.	Per cent.	
I. 1.	Organic matter alone,	−0·054	−11·11	0·0118
I. 2.	Organic matter and potash,	+0·074	+15·22	0·0301
I. 3.	Organic matter and gypsum,	−0·0302	− 6·21	0·1651
I. 4.	Org. matter, potash and gypsum,	−0·063	−13·09	0·0907
II. 1.	Organic matter,	+0·0067	+ 1·48	0·0614
II. 2.	Organic matter and potash,	+0·0876	+19·34	0·0255
II. 3.	Organic matter and gypsum,	−0·0052	− 1·14	0·0784
II. 4.	Org. matter, potash, and gypsum,	−0·0088	− 1·94	0·0479

By an inspection of this table we see:

1st. That with the exception of I. 2 and II. 2, there is a loss of nitrogen in every case (the slight gain in II. 1 being within the errors of experiment.)

2d. That this loss is very much less in the second set of experiments, where only traces of oxygen were present.

This result agrees with those of Lawes, Gilbert, and Pugh (Phil. Trans., 1865, ii, p. 509), and goes to show that the loss of nitrogen is caused by a process of oxidation. The effect of the gypsum seems to be to hinder the action. There seems to be no obvious relation between the circumstances of the experiments and the amount of ammonia formed.

3d. That in the two experiments in which the organic matter was mixed with potash a considerable gain of nitrogen took place, which was greater in the case in which only traces of oxygen were present. These two mixtures were carefully tested for nitric acid, but no trace of it was found. This fact, taken in connection with the greater gain of nitrogen in the absence of free oxygen, appears to show that nitrification is not the only means by which the nitrogen content of organic matter may be increased, and this conclusion is supported by the results of Dehérain already referred to.

* Including that of the ammonia.

This author obtained a considerable fixation of nitrogen by glucose and other organic substances mixed with alkali, in the absence of free oxygen or at least of a sufficient quantity to have formed nitric acid with even a small part of the nitrogen absorbed; and though he makes no mention of any direct test for nitric acid, it seems impossible that the gain of nitrogen observed by him could have been due to nitrification.

It has been proved by Will (Ann. Chem. Pharm., xlv, 106) that ammonia is not formed by the union of free nitrogen with nascent hydrogen; hence the gain of nitrogen in the experiments described above is not due to that reaction with the nascent hydrogen produced in decomposition.

We must then conclude that *decaying organic substances, in the presence of caustic alkali, are able to fix free nitrogen without the gain being manifest as nitric acid or ammonia, and probably without the formation of these bodies.*

In further support of this view may be mentioned the results of Boussingault's investigations on this subject (Agr., &c., t. i, p. 318). He several times obtained a gain of total nitrogen by a sample of soil, greater than that gained both as ammonia and nitric acid, thus showing either a gain of nitrogen in other forms than these, or a reversion of these to inert forms in the soil The latter explanation can hardly be accepted, however, for the results of the above described experiments, nor for Dehérain's, since the greater gain took place in the almost entire absence of oxygen; a fact which favors the supposition that no nitric acid was formed.

In conclusion, I wish to express my obligations to Prof. S. W. Johnson, at whose suggestion these experiments were undertaken, for the use of materials and apparatus, and for many valuable suggestions in regard to the conduct of the experiments.

ART. XXX.—*On the Molecular Heat of Similar Compounds;* by FRANK WIGGLESWORTH CLARKE, S.B., Professor of Chemistry and Physics in the University of Cincinnati.

IT is commonly stated in the text-books of physics that similar compounds have equal molecular heats. Thus, for the chlorides of the general formula MCl, the product of the specific heat into the atomic weight gives approximately a single value. But the equality, under ordinary circumstances, is only approximate, as a few examples will show.

Taking the chlorides of the alkaline metals, we have the following good determinations of specific heat: LiCl, ·28213;

NaCl, ·21401; KCl, ·17295—all by Regnault; RbCl, ·112, Kopp. From these, multiplying by the atomic weights, we get the following molecular heats:

LiCl,	11·99.
NaCl,	12·52.
KCl,	12·88.
RbCl,	13·54.

Here we have a gradual increase, accompanying an increase in the atomic weights. A similar increase is found in Kopp's, Avogadro's and Neumann's determinations for NaCl and KCl, and is also manifested if we work with the average of all the reliable published determinations for the same series of bodies.

In a similar manner we can compare chlorides, bromides, and iodides. Thus we have KCl, ·17295; KBr, ·11322; KI, ·08191; all by Regnault. The molecular heat, then, is

KCl,	12·88.
KBr,	13·47.
KI,	13·60.

Again there is a slight increase accompanying the rise in atomic weight. The chloride, bromide, and iodide of silver, and the same series for lead, illustrate this still farther, although it is not worth while to cite the figures here. For sodium, starting with the fluoride, we have about the same thing, only the bromide proving, perhaps, an exception. But since only one determination of the specific heat of the bromide has ever been published, it is likely that even this compound may be brought into line.

Two series of oxides are very perfect, as follows: As_2O_3, ·12786; Sb_2O_3, ·09009; Bi_2O_3, ·06053; all by Regnault. SiO_2 (quartz), ·186; TiO_2 (rutile), ·157; SnO_2 (tinstone), ·0894; all by Kopp. These determinations give us the following molecular heats:

As_2O_3,	25·31.	SiO_2	11·16.
Sb_2O_3,	26·31.	TiO_2	12·87.
Bi_2O_3,	28·33.	SnO_2	13·40.

If, instead of single determinations, we take the mean of all the published values for the specific heat of these substances, we get a similar result, perfectly in accordance with the idea of a steady increase. The same increase is illustrated by comparing the sulphates of sodium and potassium, their nitrates, their carbonates, or their pyrophosphates. We may also compare $NaNO_3$ with $NaPO_3$, and show that the latter substance has a molecular heat somewhat higher than the former. In short, by means of very complete tables of specific heat, I have been able to demonstrate the fact of this increase in over twenty series of compounds. Exceptions (even seeming exceptions)

are rare, and occur for the most part when there are but single determinations of specific heat for the apparently irregular substances. Still it is possible that there may be series of compounds in which equality exists, or in which there may be even a decrease corresponding to an increase in the atomic weights. One more series will suffice to illustrate the usual rule, and I cite this because it is the one most frequently quoted in the text-books to show the supposed equality. Instead of citing single determinations of specific heat, I will here give the results obtained from an average of all the published values:

$CaCO_3$,	21·09.
$SrCO_3$,	21·34.
$BaCO_3$,	21·49.
$PbCO_3$,	21·91.

The same regularity holds if, instead of taking an average of all, we take simply Regnault's or Kopp's determinations alone.

The fact that the molecular heat increases with the atomic weight holds good for liquids as well as for solids, and is here even more striking. Three series will serve my purpose in this connection. For the specific heats themselves I will refer to my tables of specific heat soon to be published by the Smithsonian Institution, merely stating that I have used Regnault's determinations for all the substances except CCl_4, for which Hirn's value has been employed.

		CCl_4,	31·91.
		$SiCl_4$,	32·42.
		$TiCl_4$,	35·25.
		$SnCl_4$,	37·15.
PCl_3,	27·62.	C_2H_5Br,	23·44.
$AsCl_3$,	31·95.	C_2H_5I,	24·58.

It may then be stated as a general rule, to which the present evidence offers only a few exceptions, *that in any definite series of similar solid or liquid compounds the molecular heat increases with the atomic weight, although in a very different ratio.* I have tried to establish a similar relation among the elements, but thus far without success. The results here are extremely discordant.

Now what is the meaning of this regularity? Is Dulong and Petit's law set aside by it? Speaking for solid substances alone, I should answer, certainly not. The divergencies from equality are easily explained. The specific heat of a substance varies with the temperature, generally increasing as the temperature rises. But the rate of increase is very different for different bodies. The specific heat of carbon increases very rapidly, that of platinum with extreme slowness. So, then, in

order to demonstrate Dulong and Petit's law, we must compare specific heats taken not at the *same* temperature, but at *corresponding* temperatures. This gradual increase to which I have called attention seems, then, to indicate a regularity in these corresponding temperatures, rather than an irregularity in Dulong and Petit's law. Taking our series of alkaline chlorides as an example, we may fairly suppose that their melting points (which have not been measured) are related to each other as the melting points of the metals contained in them. Of these, lithium has the highest melting point, potassium next, then sodium, and rubidium the lowest. If the melting points are the corresponding temperatures for solids, and we determine specific heats at one temperature, say at 20°, we shall have a value for lithium taken at a great distance from its proper temperature, one for potassium at a less distance, one for sodium still closer, and that for rubidium nearest of all. Then we should find that the substance having the lowest melting point possessed the highest molecular heat, a result very naturally to be expected. It seems probable, therefore, that a careful investigation side by side of specific heats and melting points would lead to the discovery of some direct relation between these two sets of constants. For specific heat much new material is needed. For melting points there are but few valuable determinations extant.

Art. XXXI.—*Relation between the Barometric Gradient and the Velocity of the Wind;* by Wm. Ferrel.

(Read before the Philosophical Society of Washington in June, and also the American Association for the Advancement of Science in August, 1874.)

1. Let G = the barometric gradient in the direction in which it is the steepest, estimated by the amount of change in the mercurial column in the distance of 100 miles;
   v = the velocity of the wind per hour;
   r = the radius of curvature of the isobar or line of equal barometric pressure;
   i = the inclination of the direction of the wind to the isobar on the side of lowest pressure;
   n = the earth's hourly angular velocity of rotation in terms of the radius;
   l = the latitude of the place;
   P = the barometric pressure of the atmosphere;
   P′ = the value of P at the earth's surface.

The following equation then expresses very nearly the relation in all cases between the barometric gradient and the velocity of the wind:

$$(1) \qquad G=\frac{(2n \sin l+u)v \sec i}{8300000} \cdot \frac{P}{P'}=\frac{(0{\cdot}524 \sin l+u)v \sec i}{8300000} \cdot \frac{P}{P'},$$

in which

$$(2) \qquad u=\frac{v \cos i}{r}.$$

This relation expresses a general law which is of as much importance in meteorology, so far as the barometric pressures and the velocities of the winds are concerned, as Kepler's laws were in astronomy, and must hold for all latitudes from the equator to the poles both at sea and on land, and for all altitudes from the earth's surface even beyond the region of the cirrus clouds. It may be applied to the mean constant motions and pressures of the atmosphere depending upon the mean difference of temperature between the equator and the poles, unaffected by the seasons, which gives rise to two grand hemispherical cyclones, of which the poles are the centers, and of which the cyclonic motions consist of the approximately eastward motions in the middle latitudes and the trade winds in the torrid zones, and also to the ordinary cyclones of comparatively limited extent, including the violent tornados and waterspouts, all of which are cyclones contained within the larger cyclones and controled by their motions. It is likewise applicable to the resultant of any number of cyclones contained within, or interfering with, one another.

2. From (2) it is seen that u is the angular velocity of gyration around the center of curvature of the isobar, which in a perfect cyclone becomes the gyratory velocity around the center of the cyclone. But in connection with this gyratory motion there is generally a motion either toward or from the center of the cyclone, toward the center below and from the center above in an ordinary cyclone, giving rise to a spiral motion, and hence at a certain height the motion toward or from the center must vanish and the gyrations be circular. The value of i, therefore, has different signs below and above.

The value of i depends mostly upon friction, but in some measure also upon the inertia of the atmosphere in cases where the motions are either increasing or decreasing, as in the beginning or dying away of the cyclone. Its value, therefore, is greatest on land and near the earth's surface, and comparatively small at sea or anywhere in the upper regions of the air. Its value also depends upon the sine of the latitude, and is equal to 90° at the equator, where $\sin l=0$, since there can be no gyrations there, and the motions must be either toward or from a center of rarefaction or condensation.

In a perfect cyclone the isobars are circular and r becomes the radius of the circle, and in this case the gradients are estimated in the direction of the radius from the center. In the two polar hemispherical cyclones the gradients are estimated in the direction of the meridians, and the isobars, supposing the cyclones to be perfect and unaffected by local disturbing causes, correspond with the parallels of latitude. In this case r in (2) is the distance from the earth's axis, and $v \cos i$ expresses the component of motion relative to the earth's surface in the direction of the parallels of latitude. In order to have the expression of G (1) strictly applicable to the polar cyclones we should have $u \sin l$ instead of u, but the value of u in comparison with $2n \sin l$, in this case, is so small that u may be neglected entirely without sensible error, so that the expression may be regarded as applicable either to the polar cyclones, or to any ordinary cyclone, or to the resultant of any number of these cyclones interfering with one another.

3. In ordinary cyclones or tornados, in which circumstances may be such as to give a very great gyratory motion near the center, the term in (1) depending upon u cannot in general be neglected, and it may even become much greater than the term depending upon n, that is, upon the earth's rotation, and in violent tornados and waterspouts, in which there are rapid gyrations very near the center, the value of the term depending upon u may be so much greater than that depending upon n, that the latter may be entirely neglected in comparison with the former. For instance, if the distance r from the center of the cyclone should be 400 miles and the velocity v equal to 40 miles per hour, if we regard the value of i as being not very large, (2) would give $u=0{\cdot}1$, which in middle latitudes would be about one fourth of $0{\cdot}524 \sin l$, and its omission would give rise to considerable error. And if the distance from the center were only 100 miles and the velocity of the wind the same, the two terms in the expression of G would be about equal, and for very small distances from the center with a large value of v, it is readily seen that the term containing n and depending upon the earth's rotation may be very small in comparison with the other. In large cyclones, however, and at a considerable distance from the center, the value of G depends mostly upon the effect of the earth's rotation, and only in some measure upon the mere centrifugal force arising from the gyrations relative to the earth's surface around the center, and depending upon the term containing u merely and not n.

4. The preceding general law expressed by (1) is deduced from carrying out more in detail principles which the writer has already had published at different times and places, but a complete demonstration of the law would be too complex and

require too much mathematical analysis to be given here. It may, however, be important to give the following explanation, rather than a complete demonstration, of this law so far as it applies to ordinary cyclones of not very great extent, especially as the method of presenting the matter here is different from any which has heretofore been used.

If water in a basin at rest has a motion of gyration around the center, the mere centrifugal force arising from the gyrations causes the water to recede from the center and produce a gradient in the surface of the water, and consequently in the pressure of the fluid upon the bottom of the vessel. If we let r represent the distance from the center and u the angular velocity of gyration, the centrifugal force is expressed by ru^2 simply. But if in addition to the gyratory motion of the water in the basin, the basin itself also has a gyration like a dish around its center, of which the angular velocity is represented by n', we then have for the whole centrifugal force

$$r(n'+u)^2=r(n'^2+2n'u+u^2),$$

and the gradient referred to the level of the water at rest in the basin, also at rest, depends upon this force. But if we refer the gradient to the level of the water at rest relatively to the basin having the gyratory velocity n', we must neglect the first term in the second member above depending upon n' merely, and the gradient then depends merely upon

$$r(2n'u+u^2)=(2n'+u)v,$$

putting $v=ru$ for the lineal velocity of motion or gyration in this case.

5. In the case of a cyclone upon the earth's surface of such extent only that the curvature of the surface can be neglected, we have exactly a similar case. In addition to the gyratory motion of the cyclone relative to the earth's surface, it is well known that the part of the earth's surface occupied by the cyclone has a motion, one component of which is that of the gyration of a disk around its center. At the poles of the earth the angular velocity of this gyration is that of the earth's rotation around its axis represented by n, but for any place between the poles and the equator it is equal to the velocity of the earth's rotation multiplied into the sine of the latitude, that is, equal to $n \sin l$. In this case we have $n \sin l$ corresponding to n' in the case of the basin of water, and hence we have $(2n \sin l + u)v$ for the centrifugal force upon which the gradient of the cyclone depends, for the term in this case depending upon $n^2 \sin^2 l$, corresponding to n^2 in the case of the basin of water, must be neglected, as in that case, since the atmospheric gradient is referred to the elliptic surface of the earth, and not to the surface in the case of no rotation around its axis. This

corresponds with the centrifugal force in (1), upon which the gradient depends in the case of no friction, in which the gyrations are circular, as supposed in the basin of water, and in which consequently $\sec i = 1$.

6. If we put

h = the height of any stratum of the atmosphere of equal pressure;

ρ = the density of this stratum;

ρ' = the value of ρ at the earth's surface;

we shall have

$$\frac{D_r P}{\rho} = g D_r h, \tag{4}$$

and, putting a for 100 miles, we get, according to the definition of G,

$$G = \frac{a D_r h}{10500} \cdot \frac{\rho}{\rho'}, \tag{5}$$

in which 10500 is the ratio very nearly between the density of the atmosphere and of mercury at the surface of the earth, where the atmospheric pressure is supposed to be 30 inches and the temperature equal to 32° of Fahrenheit. When the mean temperature of the atmosphere is greater, the value of this constant should be increased $\frac{1}{500}$ for each degree of temperature.

With the value of $D_r h$ obtained from (5) we get from (4)

$$\frac{D_r P}{\rho} = 10500 \frac{g}{a} G \cdot \frac{\rho'}{\rho}.$$

But the first member of this equation is the expression of the horizontal accelerating force arising from a difference of pressure, and this in the case of no motion either toward or from the center of the cyclone, as in the case of the basin of water, must be equal to the part of the centrifugal force causing the gradient, which, when the gradient is referred to the elliptic surface of the earth, we have seen, is $(2n \sin l + u)v$. Hence we get in this case

$$\frac{D_r P}{\rho} = 10500 \frac{g}{a} G \cdot \frac{\rho'}{\rho} = (2n \sin l + u)v.$$

Where there is motion toward or from the center of the cyclone we must add a term, F, to the last member of this equation, to represent the frictional resistance to the motion, and likewise one to represent the inertia of the air where its motion is either accelerated or retarded. On account of the extreme mobility of the air this last term may be generally neglected without any sensible error, in any of the usual motions of the atmosphere, for it can be shown that only a very small part of

the observed barometric gradients are necessary to overcome the inertia in accelerating and retarding the observed velocities. Neglecting, therefore, this effect, and adding F to the last member of the preceding equation, we get

$$G=\frac{g}{a}\cdot\frac{(2n\sin l+u)v\cos i+F}{10500}\cdot\frac{\rho}{\rho'}. \tag{6}$$

Since the motion is now spiral and not circular, and the centrifugal force depends simply upon the component belonging to circular motion, we must use here $v\cos i$ instead of v simply in the preceding expression.

7. By the principle of the preservation of areas in the case of central forces only and no friction, we would have in all parts of the cyclone

$$r^2(n\sin l+u)=\text{constant}.$$

Hence we get by differentiation

$$-rD_t u=2(n\sin l+u)D_t r.$$

The second member of this equation expresses the force which tends to produce a gyratory motion around the center of the cyclone. In the case of no friction this force is all spent in accelerating or retarding the gyratory velocity as the particles of air approach or recede from the center, but where there is friction, it is mostly spent in overcoming the frictional resistance. We shall, therefore, have very nearly

$$F'=2(n\sin l+u)D_t r, \tag{7}$$

putting F′ for the resistance to motion at right angles to the radius, or in the direction of the gyratory motion.

The resistance to the horizontal motion of any stratum of atmosphere which has to be overcome by the existing forces consists in the difference of the actions through friction of the stratum immediately above and below that stratum, so that when the relative velocity of either two contiguous strata is the same there is no resistance to be overcome by the forces. In the motion of the winds the velocity of the lower stratum relative to the earth's surface is greatest, the velocity of the second relative to the first a little less, and so on, until at a moderate height above the earth's surface the relative velocities, and consequently the resistance of friction, are very small. The frictional resistance, therefore, which has to be considered, belonging to each stratum or particle, is principally near the earth's surface, and at a small distance above it becomes comparatively small. Near the earth's surface, where the velocities of the strata increase with the height, this resistance is in the contrary direction of the motion of the atmosphere, and the component of this resistance, contrary to the direction of the gyratory motion, of which the velocity is $ru\cos i$, is represented by F′.

The other component of the resistance, therefore, contrary to the direction of the radius, in which the velocity of motion is $D_t r$, is represented by

$$F=F' \cdot \frac{D_t r}{ru \cos i}=\frac{2(n \sin l+u)(D_t r)^2}{ru \cos i}.$$

This expression is always positive, but it applies only to the part near the earth's surface, where the one component of motion is toward the center of the cyclone.

With this value of F (6) gives, neglecting $\frac{1}{2}u$ in comparison with $n \sin l$ in the value of F′,

$$(8) \qquad G=\frac{a}{g} \cdot \frac{(2n \sin l+u)v \cos i}{10500}\left(1+\frac{(D_t r)^2}{(v \cos i)^2}\right) \cdot \frac{\rho}{\rho'},$$

$$=\frac{a}{g} \cdot \frac{(2n \sin l+u)v \sec i}{10500} \cdot \frac{\rho}{\rho'},$$

since by the definition of i we have

$$(9) \qquad \tan i=-\frac{D_t r}{v \cos i}.$$

Since the unit of time is one hour in the expression of G, we must put $g=3600^2 \times 32{\cdot}2$ ft.$=79040$ miles.

With this value of g, putting $a=100$ miles, and with the value of n, the angular rotatory velocity per hour of the earth's rotation in terms of the radius, which is 0·262, we get from (8) by putting $\frac{\rho}{\rho'}=\frac{P}{P'}$, the expression of G in (1). At the earth's surface the factor $\frac{P}{P'}$ becomes unity.

8. If the internal and external parts of the cyclone had the same temperature, the strata of equal pressures would be parallel, or equidistant, and $D_r h$ would be a constant for the same place at all altitudes, and G would be proportional to S. But in order to keep up the motive power of the cyclone there must be a difference of temperature between the internal and external parts, and this causes an increase or decrease in the value of $D_r h$ with the altitude, and consequently by (5) an increase or decrease of G in a greater ratio than that of ρ, other things remaining the same. In an ordinary cyclone, in which the temperature is greatest, and consequently the density least, in the interior of the cyclone, the value of G increases in a less ratio than ρ, and hence in order to satisfy (8) $v \cos i$ must have a less value above than below; but the reverse of this is true where the density is greatest in the interior at the same pressure, as in the polar hemispherical cyclones, and hence the mean constant motions of the upper strata of the atmosphere relative to the lower ones is eastward in all latitudes.

9. The value of i in (8) depends upon friction, and its value can only be determined from observation. If in (7) F′ vanishes, $D_t r$ must vanish, and consequently by (9) i vanishes and the gyrations are circular. The greater the value of F′ for the same velocity or value of v, the greater must be the value of $D_t r$, and consequently of i. If for different velocities friction increases as the velocity, $D_t r$ must increase as the velocity increases, in order to satisfy (7), and hence (9) in this case gives i a constant for all velocities, so long as u can be neglected in comparison with $n \sin l$ in (7), but near the center of the cyclone, where u becomes very large in comparison with $n \sin l$, $D_t r$ must become very small, other things remaining the same, and hence i must also become small. For all ordinary winds it is probable that the value of i is nearly constant for all velocities, and if so, being once determined from observation for a given kind of surface, as that of the sea, or of any kind of land surface, as that of a prairie, this value is probably applicable to cyclones of all degrees of violence upon such surface, except near the center, and may be regarded as a known and fixed constant for that kind of surface.

10. Some very important work has been done by Rev. Clement Ley to determine the value of this constant, the results of which have been given in the Journal of the Scottish Meteorological Society for the first quarter of 1873, p. 66. The following mean values of i, given in connection with the several stations in the following table, were deduced from a considerable number of observations taken indiscriminately by comparing the directions of the wind with that of the isobars, as given by the signal service of the several countries to which the stations belong:

Scarborough,	4° 58′	Thurso,	15° 4′	Nottingham,	27° 44′
Brest,	7 25	Holyhead,	18 4	Oxford,	29 12
Scilly,	10 1	Aberdeen,	21 3	Brussels,	29 57
Yarmouth,	13 49	London,	21 7	Paris,	36 23
Pembroke,	14 47	Greencastle,	22 1	Skudnesnaes,	41 17

From these results Mr. Ley arrived at the following conclusions:

I. The winds commonly incline from the districts of higher toward those of lower pressure. The collective mean for the 15 stations is 20° 51′.

II. This inclination is much greater at inland than at well exposed coast stations. The collective mean for Brest, Scilly, Yarmouth, Pembroke and Holyhead is 12° 49′, while for the internal stations, London, Nottingham, Oxford, Brussels and Paris, it is 28° 53′.

These results exactly confirm the theory of ordinary cyclones, which requires, where there is friction, that there should be a motion of the air below toward the center of the cyclone, as

well as a motion of gyration, and hence i must have a positive value. This, it is seen, is obtained from observation for each one of the 15 stations taken separately. Moreover, the inland stations, where the resistances are greater, give a greater value of i than the stations on the sea coast, where the resistances are smaller. This likewise accords with theory.

From the small value of i obtained by Mr. Ley for the coast stations, we may infer that at sea, and likewise in the upper regions of the atmosphere, it is still considerably less. The value of i, therefore, in (8), except at internal stations where the resistances are great, may be regarded so small that its secant can be taken as unity, and hence either the gradient or the velocity of the wind is known the one from the other. For inland stations near the surface, where the resistances are great, the value of i must be obtained from observation.

11. With regard to the inclinations of the winds to the isobars belonging to the different quarters of the compass, Mr. Ley obtained for the inclination of S.E. winds 35° 11′, of N.E. winds 17° 43′, of N.W. winds 9° 4′, of S.W. winds 20° 13′. In these results S. winds were taken as S.W. winds, E. winds as S.E. winds, &c. Hence E.S.E winds have the greatest, and W.N.W. winds the least inclinations to the isobars, which correspond very nearly to the N.E. and S.W. sides of the cyclones. Mr. Ley states that the average direction of the cylones was about N.E. Hence it appears that the inclination of the front part of the cyclone is greatest, and that of the rear the least, and this may perhaps be found to be a general law. It was also found that the differences in the inclinations of the winds from the different quarters is greatest at coast stations.

Mr. Ley likewise obtained the very important result that the difference between the inclination of strong and light winds at stations on the coast is trivial, but at inland stations the mean inclination is less with strong than with light winds, and that at all stations the inclination is more regular with a gale than with a light wind. This is all exactly in accordance with the preceding theory, for we have just seen in a preceding paragraph that where the value of u in (7) becomes large, as it does near the center of a cyclone, the value of i must become less; but the strongest winds are also near the center of the cyclone, and hence, in general, the strongest winds are found to have the least inclinations to the isobars. And as the tendency of the very rapid gyrations near the center is to approximate to a circular gyration, it is evident that the inclinations must be more regular with such winds, which are generally gales, than with light winds; as found to be the case by Mr. Ley from observation.

12. If the water in a basin of water has an interchanging

motion between the internal and external parts, and this basin itself has a horizontal gyratory motion around its center, it is well known that the water toward the center, by the principle of the preservation of areas, tends to run into rapid relative gyrations, but if the basin has no such gyratory motion, the water does not run into such gyrations, but simply keeps the gyrations belonging to the basin. So if, by means of the rarefaction of some area of the atmosphere, so as to cause a difference of density between the internal and external parts, an interchanging motion is kept up between these parts, the atmosphere must run into gyrations, that is, give rise to a cyclone, if the area occupied by this part of the atmosphere has a gyration around its center, as we know it has, unless this center is on the equator. The value of u, therefore, in (7) depends upon the amount of gyratory motion which the part of the surface of the earth occupied by the cyclone has around its center, and vanishes at the equator where this gyratory motion vanishes. The factor $(n \sin l + u)$, therefore, in the second member of (7), depends upon the latitude of the place, and vanishes at the equator. Hence the greater the latitude the less must be the value of $D_t r$, when $v \cos i$ and consequently F′, the resistance to this component of the motion, remain the same; and therefore by (9) the less must be the value of i. At the equator also, where $v \cos i$ vanishes on account of the vanishing of the gyrations, $\tan i$ becomes infinite, and consequently $i = 90°$.

13. Mr. Ley's results obtained from the averages of all the stations correspond to about the parallel of 50°. We now see that, according to theory, the less the latitude the greater must be the value of i obtained from observation, and that at the equator the motions would be found at right angles to the isobars. It would, therefore, be interesting to have the value of i obtained from observations for other latitudes. This has been done recently by Professor Loomis (this Journal, July, 1874), from two years' observations of the Signal Service of the United States, as given in one of the tri-daily weather maps for each day. The method adopted is different from that used by Mr. Ley, but the results obtained should be the same by each method. The angle of inclination obtained from these weather maps was over 45°, which is more than one-third of this value greater than that obtained by Mr. Ley for the five inland stations. The average latitude of the stations on the United States weather maps is considerably less than that of the five inland stations used by Mr. Ley, and a part of the preceding difference is, no doubt, due to that cause, as theory requires, but perhaps the greater part is due to the greater amount of frictional resistance to the gyrations in a new and wooded country.

In the case of the trade winds, a part of the polar cyclones, the inclination of the direction of the wind to the isobars at sea is about 45°, and this being at about the latitude of 20°, the value of this angle, by theory, should be very much greater than its value at sea at the parallel of 50°, which from Mr. Ley's small value obtained for the coast stations is perhaps less than 10°, so that this also confirms the preceding theory.

14. Having now learned something from both observation and theory with regard to the value of the theoretically unknown angle i contained in the relation expressed by (1), we shall now proceed to make some comparisons of this law or relation with observation. We shall first consider the mean gradients and velocities belonging to the two polar hemispherical cyclones. Observation shows that the barometric pressure is a maximum, and that, consequently, G vanishes about the parallel of 35° in the northern hemisphere, and a little nearer the equator in the southern hemisphere, and that there are calm belts, called the tropical calm belts, at these latitudes, except so far as they are occasionally disturbed by local cyclones. By the relation expressed by (1), if we put $G=0$, we must also have $v=0$, unless $\cos i$ vanishes, which, we have seen, does not, except at the equator. There must, therefore, be a calm where $G=0$, and hence we have the tropical calm belts. At the equator we also have $G=0$, and with this value (1) is satisfied with $v=0$, that is, with a calm belt, as observed at the equator; but it is likewise satisfied by $(2n \sin l+u)$ vanishing at the equator, and hence v is arbitrary.

Again, observation shows that the mean constant barometric pressure increases in the middle latitudes in going from the poles to the equator, and decreases within the tropics, and hence G is positive in the former case and negative in the latter. The relation of (1), or of (8), requires that in the former case we should have the principal component of velocity, $v \cos i$, positive, and in the latter case negative, that is, that in the middle latitudes there should be an eastward motion, and between the tropics and the equator a motion toward the west. This, it is well known, is in accordance with observation.

15. The barometric pressures given by Professor Loomis (Meteorology, p. 18) indicates that at about the parallel of 64° in the northern hemisphere, and about the parallel of 74° in the southern hemisphere, there is a minimum of barometric pressure, and hence G vanishes in these latitudes, and therefore, for reasons which have already been given, we must have calm belts there. The observations from which these pressures have been deduced perhaps did not sufficiently embrace all longitudes and all seasons to give the mean constant pressures, unaffected by local circumstances and the sea-

sons. This we know is the case in the southern hemisphere, where the most southern observations, obtained mostly by the British Board of Trade, were necessarily made during the summer season, when the barometric pressure is the greatest in those latitudes. If, however, it can be clearly shown by observation that the mean annual pressure is a minimum at these latitudes all around the globe, then the gradient or value of G is negative between these parallels and the poles, and there must consequently be a wind between these latitudes and the poles having the component $v \cos i$ from the east, and as the other component of motion at the surface must in this case be from the pole toward the minimum pressure, the wind must be from some point between the north or south, according to the hemisphere, and the east.

16. The real velocities and directions of the mean constant winds have been determined only very roughly from observation on any part of the globe, and hence no very accurate comparisons of our law with observation can be made. Such comparisons, however, all seem to establish the truth of the law within the limits of the errors of observation. The mean constant isobars in the British Islands, as determined by Mr. Glaisher, all effects of the seasons and of local disturbing causes being eliminated, gives very nearly $G=0{\cdot}02$ of an inch. With this value in (1), supposing the direction of the wind to be toward the east, or nearly so, or that the value of i is small, we get $v=6$ miles nearly. This is a very little less than the mean eastward velocity of the wind here, as determined by the late Prof. Coffin, and given in his "Winds of the Northern Hemisphere." Again, from the table of barometric pressures given by Buchan (Handy Book of Meteorology, p. 27) we deduce the approximate value of $G=-0{\cdot}02$ about the parallel of 20° in the northern hemisphere. The stations in this table belong to widely different longitudes, and the gradients, no doubt, differ considerably on the same parallel in different longitudes, so that this is merely a rough average value for that latitude. On the ocean in this latitude, where the trade winds prevail, without any disturbance from local disturbing causes, the direction of the wind being about N E., is inclined to the isobars about 45°, and the direction being westward the value of i in this case is about $180°+45°=225°$. With this value of i, and the value of G above, we get from (1) $v=15$ miles nearly, the principal component $v \cos i$ being negative and consequently westward in this case. This velocity cannot differ very much from the usual velocity of the trade winds, so that the result seems to confirm our law with regard to velocities.

From the table of barometric pressures given by Buchan, which has been already referred to, we obtain for the parallel

of 52° in the southern hemisphere the value of G=0·07 of an inch, and the tabular results from which this is obtained are the averages of a very large number of observations obtained by the British Board of Trade, so that this gradient may be regarded as being pretty accurate, and it is perhaps very nearly the same on that parallel all around the globe. Supposing the value of i here to be small, that is, that the wind blows very nearly from the west, we get from (1), with this value of G, v =21 miles for the velocity of the winds on this parallel. All accounts represent the west winds in this latitude as being very strong all around the globe. Mr. Laughton says:* "Between the parallels of 40° and 60° the westerly wind blows with a constancy little inferior to that of the trade, but with much more violence. About the parallel of 50°, indeed, it is found, as a rule, to be blowing 'half a gale of wind,' and this not only south of the Atlantic, but all round the world." A velocity of 21 miles per hour makes a pretty strong constant wind, and this theoretical velocity probably corresponds very nearly with the average wind in this latitude.

17. We come now to make some applications of the law to ordinary cyclones. Since in the center of a cyclone the barometric pressure must be a minimum and consequently G=0, in order to satisfy (1) in this case we must have v=0, and hence there must be a calm at the center. In the large cyclones the gradient or value of G may not become sensible for a considerable distance from the center, and in such cases there is no sensible velocity of the wind within that distance. The area of almost a perfect calm in some cyclones is said to be as much as 30 miles in diameter.

The gyrations of the external part of a cyclone are necessarily in the contrary direction, and hence the component of gyratory motion $v \cos i$ must be negative, and consequently sec v in (1), and the sign of G becomes reversed. At some distance, therefore, from the center of a perfect cyclone between the center and the outer limit, the barometric pressure must be a maximum and G vanish. At this distance, therefore, by our law we have v=0, that is, a calm. Hence areas of high barometer must generally be areas of calms.

18. If the isobars of a cyclone drawn to every tenth of an inch of the barometer reduced to sea level are 100 miles apart, we have G=0·1 of an inch. With this value of G, supposing the value of i to be so small that we can put sec i=1, we get from (1) at the distance of 400 miles from the center of the cyclone, or center of curvature of the isobars, and on the parallel of 45°, v=29 miles for the velocity of the wind; and this would be very nearly the actual velocity at sea, where the

* Physical Geography in its Relations to the prevailing Winds and Currents, p. 41.

value of i is small. But if the value of i is 45°, which is nearly the value obtained by Professor Loomis from the average of all the stations of the United States Signal Service, then we get for the velocity of the wind, under the same circumstances, $v=21$ miles. With a still much greater inclination than this average inclination, which must frequently happen, this velocity becomes much less.

At a distance of only 100 miles from the center, all the other circumstances being the same as above, we get $v=22$ miles in the case in which i is small, as at sea; and in the case in which the value of i is 45°, we get $v=18$ miles nearly. The comparison of these values of v with those of the preceding paragraph shows that the velocity of the wind belonging to the same gradient diminishes considerably toward the center of the cyclone.

19. The preceding law or relation contained in (1) cannot be tested by comparing the observed velocities of the wind in individual cases with those deduced from the corresponding gradients obtained from the isobars as laid down on the weather maps of the Signal Service; for these being laid down from observations made at stations which are in many cases several hundred miles apart, the effects of the minor more local disturbances cannot be taken fully into account, since within an ordinary cyclone there may exist several smaller cyclones with distinct centers of their own, of so small extent that their effects upon the isobars cannot be determined from stations merely at wide distances apart. The law can only be tested by comparing the average velocity of a great many individual cases with the average of the corresponding observed gradients, as deduced from the isobars, taking account of the distances of the stations from the centers of curvature of the isobars.

20. According to the empirical law of Dr. Buys Ballot, the wind blows at right angles to the line joining the highest and lowest pressures, or, in other words, in the direction of the isobars, and with a force proportional to the steepness of the gradient. This, so far as it represents the true law of nature, is wholly contained within the preceding theoretical and much more general law. Theoretically the direction of the wind can never exactly coincide with that of the isobars, but according to the results obtained by Mr. Ley the inclination to this direction may be small in the higher latitudes at sea, and on level prairie or mostly cultivated countries, where the winds are not obstructed by wood-lands; but according to our theoretical law this angle, even at sea, must become large toward the equator, and accordingly we find that the trade winds at the parallel of about 20° are inclined to the direction of the isobars about 45°. This law of direction then, as a general law applicable to all latitudes, is not even ap-

proximately true near the equator. With regard to the force or velocity of the wind, we see from a mere inspection of the expression of G in (1), that the velocity in all parts of the cyclone for the same latitude is not proportional to G, but is less near the center of a cyclone; and this has likewise been shown (§17) by obtaining numerical results in special assumed cases. It is, moreover, seen that in different latitudes the value of v corresponding to the same gradient, or value of G, must be nearly inversely as the sine of the latitude, especially at a considerable distance from the center of a cyclone, where u in (1) is small in comparison with $2n \sin l$. For this reason the violence of the wind in a cyclone corresponding to the same gradient is much greater within the tropics than in the higher latitudes.

21. If we put

G′ = G divided by 100 miles;

D = the barometric depression in inches at the earth's surface in the middle of a cyclone;

we shall have

$$(10) \qquad D = \int_r G' = \int_r \frac{(0{\cdot}524 \sin l + u) v \sec i}{830000000},$$

in which v must be expressed in miles, and the integration be made from $r=0$ to r equal its value when the barometric pressure is equal to some assumed value, as the mean, below which the depression is estimated.

It is well known that there is a depression of the barometer below the mean in the middle of all cyclones. In the polar regions, near the centers of the two polar hemispherical cyclones, the barometric pressure is considerably below the mean, near the north pole more than a half inch, and near the south pole about an inch, the depression at the latter being greater on account of there being but little land and few mountains in the southern hemisphere to obstruct the gyrations around the pole, upon which the depression depends. In the middle, also, of ordinary cyclones a depression below the mean of two or more inches is sometimes observed. Up to this time no meteorologist has accounted for these depressions. It has been attempted to account for them by means of the centrifugal force arising from the gyrations relative to the earth's surface merely, neglecting the gyrations arising from the earth's rotation; but this in all cases gives an effect very much too small. For upon this principle we neglect the principal term in the expression of D (10) depending upon $0{\cdot}524 \sin l$, and retain only that depending upon u, which, we have seen, is generally very small in comparison with the former. In fact, in the two polar hemispherical cyclones u is so small in comparison with $0{\cdot}524 \sin l$, that it

can be entirely omitted without sensible error, so that by retaining merely this very small term we get from (10) no sensible depression of the barometer toward the poles at all. In ordinary cyclones, also, the effect depending upon u merely is very small in comparison with that depending upon the other term, except in violent tornados of small extent, in which the principal part of the gyrations is near the center. By using the complete expression of D we obtain a complete and satisfactory explanation of all these depressions. When a cyclone is of large extent no great velocity and corresponding steepness of gradient is necessary in any part to give a great depression, for the greatness of the depression arises from the extent of the integration. For instance, in the great polar cyclone of the southern hemisphere, we have seen that the greatest eastward velocity, corresponding to the steepest part of the gradient about the parallel of 52°, which is necessary to give the great depression near the south pole, is only twenty-one miles per hour.

22. All barometric oscillations depend almost entirely upon cyclonic action, and are caused generally by the passage of ordinary cyclones over the place of observation. Hence at the equator, where cyclones cannot be formed, there are scarcely any sensible barometric oscillations except the regular small diurnal oscillations. In this case the expression of D in (10) becomes indeterminate, since at the equator $(2n \sin l + u)$ vanishes, but sec i becomes infinite. But since the value of i depends upon friction and upon the neglected effect of inertia, the value of D must likewise, in this case, depend upon these. Observation shows that at and near the equator the barometric oscillations are extremely small. Col. Sykes observes with regard to the small diurnal oscillations on the plateau of the Deccan: "In many thousand observations made by myself there was not a solitary instance in which the barometer was not *higher* at 9–10 A. M. than at sunrise, *lower* at 4–5 P.M. than at 9–10 A. M., *whatever the indication of the thermometer or hygrometer might be:* nor was there a solitary instance in the year 1830 in which the maximum *night tide* was not higher than the 4–5 o'clock day tide."* Humboldt likewise observes with regard to the regularity of these oscillations in the torrid zone: "This regularity is such that, in day time especially, we may infer the hour from the height of the column of mercury without being in error, on an average, more than 15 or 17 minutes. In the torrid zone of the new continent I have found the regularity of this ebb and flow of the aerial ocean undisturbed either by storm, tempest, rain, or earthquake, both on the coasts, and at elevations of nearly 13,000 English feet above the level of the

* Phil. Trans., 1835, p. 167.

sea." These small diurnal oscillations have nothing to do with the question which we are considering, but as the range of these oscillations is only 0·1 of an inch, the preceding extracts show that the irregular oscillations due to other causes, which in the higher latitudes frequently amount to an inch or more, in the torrid zone are so small as scarcely to interfere with the regularity of these small diurnal oscillations, and hence must be themselves very small. At the equator there cannot be any gyration, and hence in the storms and tempests of which Humboldt speaks, the motion of the air at the surface of the earth is directly toward the center of the area of diminished density, and both the inertia of the air and the friction belonging to this motion are overcome by the force arising from almost insensible barometric gradients. The inertia, therefore, of the air merely, which we omitted at the outset in obtaining the relation of (1), is overcome by a very small part of the usual gradients, and its omission, consequently, gives rise to only a very small error in any case. In a cyclone the force arising from the gradients is almost exactly balanced by that arising from the centrifugal force of gyration, including that of the earth's surface, and so great is the mobility of the air, and so small is the amount of friction, that it is only a very small part of this force which is spent in keeping up the motion of the air between the internal and external parts of the cyclone.

23. If friction is as the first power of the velocity, it is evident from (7), other circumstances remaining the same, that the gyratory motion of a cyclone, and consequently the value of v, is as the sine of the latitude, or $\sin l$, and hence the value of D in (10) is as the square of this sine. The mean monthly ranges of the oscillations of the barometer must, therefore, increase with the latitude, and should be somewhat in proportion to $\sin^2 l$. This is shown to be the case from observation. In the following table we have the mean monthly ranges given by observation * corresponding to the given latitudes in the first column, and in the last column we have the ranges which increase as $\sin^2 l$, putting the range at the pole equal to 1·6 inches.

l.	Mean monthly ranges.	1·6 in. $\sin^2 l$.
0°	0·1 in.	0·0 in.
30	0·4	0·40
45	1·0	0·80
65	1·33	1·30
78	1·2	1·36

Of course we cannot expect a very nice agreement in the last two columns, since there are several circumstances besides lati-

* Loomis' Meteorology.

tude which affect these oscillations. The rapidity of the gyrations in a cyclone, and consequently the amount of barometric depression in the center, depends very much upon the amount of aqueous vapor with which it is fed, and this diminishes from the equator to the pole. The great cyclones also, which move from lower to higher latitudes, continually increase in diameter, and hence the amount of depression in the center, and the amount of barometric oscillation at any place caused by the passage of these cyclones over it, must increase toward the poles. These two effects, however, tend in some measure to counteract each other. The monthly range of 0·1 of an inch at the equator is simply that of the diurnal oscillations due to another cause, so that the irregular oscillations sensibly vanish there, as required by theory.

No adequate cause has heretofore been given for these oscillations, nor one which does not equally apply at the equator and in the higher latitudes. We have here given a complete explanation of them, and of their vanishing, or nearly so, at the equator, and of their gradually increasing with the latitude. We now also see the reason why the diurnal oscillations are so regular, not only exactly at the equator, but throughout a belt of considerable extent on each side of it. For the irregular oscillations, which interfere with the regularity of the diurnal, being as the square of the sine of the latitude, must be very small for a considerable distance from the equator. The preceding results also still further verify the law expressed in (1), since the expression of (10), upon which these results are based, is obtained directly from the former.

24. Wherever the atmosphere over any large area of the earth's surface receives a gyratory motion from any cause, this motion gives a value to u and v in (1), and, hence, likewise to G. The term, however, depending upon u, in any such case, is so small in comparison with that depending upon $2n \sin l$, that it can be omitted without sensible error. We can also put $\sec i=1$ in this case, and then we have $v=ru$, and v may be either positive or negative. If v is positive, that is, if the gyration is from right to left in the northern hemisphere, or the contrary in the southern, the value of G is positive, and the pressure increases from the center outward, and there is low barometer at the center; but if the gyrations are the contrary way in either hemisphere, the value of G is negative, and consequently *high barometer* at the center of gyration. The westward motion of the air at the parallels of the trade winds in the Atlantic Ocean, and the eastward motion in the middle latitudes, are in some measure obstructed by the continents and deflected from their course. The westward motion north of the equator is turned northward over and along the coast of

the United States, and the eastward motion in the North Atlantic is deflected south and north by the continent of Europe, while on the corresponding part of the coast of America the air is drawn away faster than it is supplied by the flow over the continent on account of the greater resistances on land than on sea, and the same occurs on the African coast in the torrid zone. Hence the deflected currents supply these deficiencies and give rise to two gyrations in the North Atlantic, the one being a gyration from left to right over a large area having its center about the parallel of 30° and half-way between the continents; the other, in the northern part of the Atlantic, being a gyration from right to left, and having its center about the parallel of 65°. Hence there is an area of high barometer in the middle of the former gyration, and of low barometer in the middle of the latter. The effect of these gyrations is indicated by the isobars drawn on the British Admiralty charts, on which several of these isobars, drawn to tenths of an inch of the barometer, always return into themselves. If we suppose these isobars to be 500 miles apart at any place, on the parallel of 30°, the value of G would be 0·02 of an inch, and with this value of G, putting $l=30°$, we get from (1) $v=8$ miles nearly for the velocity of gyration per hour at that place. In the northern gyration the velocity corresponding to the same gradient would be less on account of the greater value of $\sin l$. From the isobars of the Admiralty chart we see that there are two similar gyrations in the North Pacific, causing an area of high barometer about the latitude of 30°, and of unusually low barometer toward the pole. There are also indications of these gyrations immediately south of the equator in both the Atlantic and Pacific Oceans, and also in the Indian Ocean, as shown by the isobars, but not toward the poles, since the continents in the southern hemisphere do not extend far enough south to produce the necessary deflections there.

25. If we multiply the second member of (1) by 10,500, the ratio between the density of the air at the earth's surface and mercury, by which we get 800 nearly for the denominator instead of 8,300,000, G expresses the gradient of sea-level due to the deflecting forces of the earth's rotation. The southern part of the North Atlantic is supposed to make a gyration in about three years, on account of the more complete deflections of the continents in this case similar to those of the atmosphere. This, at the distance of 1,500 miles from the center of gyration, would give $v=-0·35$ of a mile, v being negative, since the gyrations are from left to right. With this value of v, putting $\sec i=1$, we get from (1) with the new denominator, $G=-0·58$ of a foot for the change of sea level in 100 miles on the parallel of 30°. The value of G being negative shows that the sea-

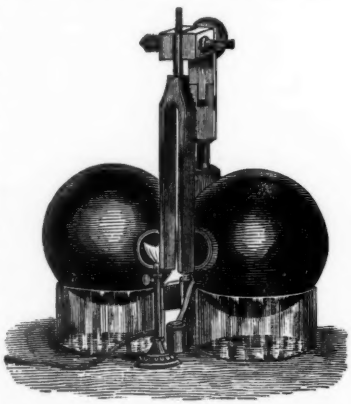
level is highest in the middle of the gyration, and the difference of sea-level between the center and the external part of the gyration would amount to several feet. Similar gradients in the sea-level are produced wherever there are ocean currents, the gradient being proportional to the velocity of the current and the sine of the latitude.

ART. XXXII.—*Researches in Acoustics;* by ALFRED M. MAYER. Paper No. 7, containing:

Experiments on the Reflection of Sound from Flames and Heated Gases.

THE reading of the recent interesting research of Prof. Tyndall on "Experimental Demonstrations of the Stoppage of Sound by partial Reflections in a non-homogeneous Atmosphere" (Proc. R. S., Jan., 1874; Nature, Jan. 29, Feb. 5), and of the subsequent paper by Mr. Cottrell "On the Division of a Sound-wave by a Layer of Flame or Heated Gas into a reflected and a transmitted Wave" (Proc. R. S., Feb. 12, 1874), caused me to turn my attention to the experimental illustration of the reflection of sonorous vibrations from flames, heated gases, and from sheets of cold gases and vapors.

The following experiments are of easy execution, and show in a marked manner the reflecting powers of sheets of flame and heated gas, and even serve to give approximate measures of these reflecting powers.



Take two similar resonators and place the planes of their mouths at a right angle; then in this angle firmly fix the fork corresponding to the resonators, so that the broad face of one of its prongs faces the mouth of one resonator, while the space between the prongs faces the mouth of the other resonator. (See the figure.)* By trial the two planes of the fork are placed at such distances from the resonators that complete interference of the vibrations

* The wood-cut illustrating this paper has been kindly given to me by Dr. Henry Wurtz, the editor of the American Gas-Light Journal, in which publication an account of these experiments first appeared—May 2, 1874.

issuing from their mouths is obtained, and the only sound that reaches the ear is the faint sound given by the fork's action on the air outside the angle included by the mouths of the resonators. If, in these circumstances, we close the mouth of either resonator with a piece of cardboard, the open resonator will strongly reinforce the sound of the fork. If we now also cover the mouth of the latter resonator with a piece of cardboard we shall again have silence. Also, if we substitute, for one of the pieces of cardboard, a slip of stout glazed note-paper, the same result is obtained. But, if we replace the piece of note-paper by a similar piece of French tracing-paper, a faint sound issues from the resonator so covered, because the tracing-paper is sufficiently permeable to sonorous vibrations to permit the resonator to slightly reinforce the sound of the fork. This reinforcement becomes greater if we substitute for the tracing-paper a piece of tissue paper—such as is used in printed books to cover steel engravings—and a yet greater reinforcement is produced when we put in the place of the tissue-paper a piece of the soft, loosely woven paper which is used by French instrument makers for the inner wrapping of their packed wares. I thus obtained a graded series of substances, more and more permeable to sonorous vibrations.

I again obtained neutralization, by interference, with the mouths of the resonators open, and then screened the mouth of one of them with a bat's-wing coal-gas flame. The vibrations issuing from the resonators were now no longer neutralized, but the vibrations from the uncovered resonator had a great ascendancy over the other, so that a strong sound issued from it. I now tried to destroy this superiority by screening its mouth successively with the graded series of paper screens. The loose, soft paper was not equal to it; nor was the tissue-paper; but the tracing-paper just equalled the effect of the gas-flame in guarding the mouth of the resonator from the entrance of sonorous vibrations. On lowering the gas-flame, so that its top luminous border was just below the mouth of the resonator—and therefore only a sheet of heated air ascended across the latter,—the balance of the tissue-paper against the hot gases and vapor remained unimpaired. Thus it appears that the reflecting power of a sheet of coal-gas flame or of a sheet of the heated carbonic acid and vapor of water just above it, exactly equals, in the above described circumstances, the reflecting power of tracing-paper.

I have also found that the passage of a sheet of cold coal-gas across the mouth of the resonator, was sufficient to destroy the balance of the interference, and caused a faint sound to issue from the other resonator; a similar effect, and nearly equal in intensity, was obtained with a sheet of cold carbonic acid gas;

while cold, dry hydrogen closed the mouth of the resonator more effectively than either of the above gases, but was far inferior in this shielding action to the sheet of heated gases above the bats-wing gas flame. I attempted to get a rough measure of the "acoustic opacity" of hydrogen, but could find no substance sufficiently permeable to balance it, except a sheet of rather fine wire gauze. We should not place too much confidence in measures of the reflecting power of surfaces made by the method just described, and which I have used merely to give approximations of the reflecting powers of the above named gaseous sheets; for the substance which closes the mouth of the resonator may allow a considerable portion of the sonorous vibrations to enter the latter, and yet the resonator may not be able to reinforce the sound by reason of its being thus thrown out of tune with the fork, by an unyielding surface closing its aperture. Thus, a sheet of thick note-paper prevents resonance as effectively as a thick piece of Bristol-board, or a plate of metal; yet we know well that these substances have very different powers to reflect sonorous vibrations. As a flat coal-gas flame equals a piece of tracing-paper in reflecting sonorous vibrations, it follows that we can substitute the former for the latter in all experiments where the presence of the paper produces, by its reflecting power, an alteration in intensity or in pitch. Thus, if we vibrate a fork before the mouth of a resonator while the nipple of the latter is open, we obtain a far inferior reinforcement to what takes place when the nipple is closed Now, the nipple can be partly closed with a gas-flame or a sheet of heated air. Thus, alternately closing and opening the nipple of an Ut_4 resonator with the flame of a Bunsen burner, gives excellent results.* The reflecting power of a bats-wing flame is also well shown by successively closing and opening the mouth of any resonant box of forks in the octave Ut_4 to Ut_5. Also, if the plug be taken out of the ends of closed organ pipes and these pipes be placed horizontally, the reflecting effect of the flame is heard when the latter is passed forward and backward across the open ends of the pipes, while the ear is placed in the axes of the pipes. The simplest method, however, is to sound the fork (either continuously by electro-magnetism, or by a bow) in front of its resonator, and successively to close and open the mouth of the latter with a flame or sheets of heated gas, or of cold vapors or gases. The contemplation of these experiments naturally calls up the question, Is the action of the flame due entirely to reflection? may it not also absorb part of the sonorous vibrations, as in the analogous phenomena of the reflection of light? If the inten-

* In all of the experiments described in this paper care was taken that no heated air or gases entered the resonators, and thereby put them out of tune.

sity of the sonorous vibrations, which have traversed the flame, equal the intensity of the vibrations which impinged on the flame minus the intensity of those which were reflected from the flame, then there is no absorption of these vibrations by the flame; but if this equality does not exist, then there is absorption in the flame; and this means that *the flame is heated by the sonorous vibrations;* which enter the flame as *sonorous vibrations*, but issue from the flame as *heat vibrations.* It thus, at first, appears that the absorption of the sonorous vibrations might be detected by their production of an increase in the temperature of the flame; just as sonorous vibrations are absorbed by caoutchouc and reappear as heat in this substance.

In the following manner I have recently made experiments in the direction of determining the equivalent of a given sonorous aerial vibration in fraction of a Joule's unit of 772 foot-pounds. I stretched between the prongs of an Ut_3 tuning-fork a piece of sheet caoutchouc, $\frac{1}{\overline{0}\,\overline{0}}$ inch in thickness, and about $\frac{1}{2}$ inch broad. The effect of this rubber on the vibrating fork is rapidly to extinguish its vibrations, while the rubber itself is heated; and if a fork be vibrated continuously by one and the same force when the rubber is stretched on it, and then when it is taken off, the aerial vibrations produced by the fork are far more intense in the latter circumstances than in the former. By a method described by me in this Journal, Feb., 1871, I measured the relative intensities of the aerial vibrations in these two conditions of vibration. The sheet of caoutchouc was enclosed in a compound thermo-battery and the fork vibrated during a known interval; the rubber was heated by the vibrations which would have appeared as sonorous vibrations if the rubber had been removed from the fork. The amount of heat given to the caoutchouc was accurately determined by the deflections of a Thomson reflecting-galvanometer connected with the thermo-battery; and by knowing the interval during which the fork vibrated, the amount of heat given to the caoutchouc during this interval, and the equivalent of the heated rubber in water, I calculated the intensity of the sonorous vibrations in terms of a thermal unit, from which I at once obtained the value of the sonorous aerial vibrations when the fork was not heating the rubber; in other words, when it vibrated freely. I thus found that the sonorous aerial vibrations, produced during ten seconds, by an Ut_3 fork placed in front of its resonator, equalled about $\frac{1}{\overline{0}\,\overline{0}\,\overline{0}\,\overline{0}\,\overline{0}}$ of a Joule's unit; that is, they can be expressed in the work done in lifting 54 grains one foot high. This quantity of heat, which is equal to the heating of one pound of water $\frac{1}{\overline{0}\,\overline{0}\,\overline{0}\,\overline{0}\,\overline{0}\,\overline{0}}$ of a degree Fahr., expressed the amount by which the gas flame would be heated if it absorbed all of the sonorous vibrations issuing from the Ut_3

resonator. But this is such a small fraction of the entire heat in the flame, that it is far within the actual fluctuations in its temperature, and even if the flame were constant in temperature, this small increase could not be detected by any known thermometric method. We cannot therefore determine the amount of absorptive power of a flame or of a sheet of heated air for sonorous vibrations, by experiments on their increased temperatures when sonorous vibrations impinge on these bodies.

Hoboken, April 10th, 1874.

ART. XXXIII.—*Velocity of Primitive Undulation;* by PLINY EARLE CHASE, Professor of Physics in Haverford College.*

NEWTON'S hypothesis of a primitive æthereal energy, as the source of cosmical attraction, underlies the fifty theses† which I have submitted as a contribution to the discussion of Anderssohn's "Lösung des Problems über den Sitz und das Wesen der Anziehung," at the coming Breslau meeting of the German Association of Naturalists and Physicians. In those theses I have embodied some of the most important conclusions to be drawn from the physical investigations in which I have been engaged during the past twelve years, and I desire to invite special attention to the curious analogy in the closing thesis, which may perhaps furnish an important clue to the unity of forces that so many are seeking, and of which Secchi treats so largely.

Henry's experiments have shown that there may be ponderable resistance occasioned by imponderable agency; Plateau's, that motion may shape cosmical masses; Anderssohn's, that so-called attractions may be explained by thrusts as well as by pulls; my own, that purely mechanical vibrations may produce polarity. The tendency of modern theorizing to resolve all physical agencies into forms of motion, suggests the inquiry whether the primitive motion, of which gravity and other forces may be special developments, has a constant, or a variable velocity. The hypothesis of constancy seems to me more probable *a priori*, and the subordinate variability, directly as the mass and inversely as the square of radius, can be readily explained in accordance with that hypothesis; inasmuch as the number of radial impulses, intercepted by a given surface, varies as the inverse square of the distance, and the inertia varies as the mass. Galileo's law of rectilinear motion, Newton's laws of motion with reference to centers of force, and the

* Read before the American Association at Hartford, Aug. 13, 1874.

† Proc. Amer. Phil. Soc., xiv, 141–7.

resulting laws of curvilinear motion under combined tangential and focal impulses, are all simple corollaries from the hypothesis of a universally oscillating, perfectly elastic, imponderable, material æther, which would necessarily establish special systems of centripetal and centrifugal undulations about every center of gross inertia. We may, therefore, reasonably seek for the value of the hypothetical constant velocity, and in order that our terms may all be strictly comparable, we will look merely at radial action, either toward the sun's center, or toward the central sun. Let

$2F = M$ = modulus of Newtonian æther = height of a homogeneous æthereal atmosphere, at sun's surface, which would propagate undulations, or motion toward a vacuum, with the primitive constant velocity = 2 × virtual fall, which would produce the same velocity.

$2r$ = diameter of sun at any given time, either actual, or when homogeneously expanded or contracted to any point in space; $r_{'} = nr_0$ being the present radius, and r_0 the radial unit.

$2r_0 = v_0$ = diameter of sun at which the æthereal and gravitating motions would all be equal, and 2F would $= 2r = v_0 = v_{'} = v_{''} = v_{'''} = 2f$; v_0 being the limit of possible æthereal velocity = primitive constant velocity, expressed in the unit of time of traversing $2r_0$.

$v_{'} = \sqrt{2gr} = 2\sqrt{\frac{f}{r}}$ = velocity communicated by infinite fall to sun's surface, (present, past or future; actual, expanded or contracted).

$v_{''} = \frac{2}{\pi} v_\rho = \frac{4r}{t_\rho}$ = mean velocity of grand-central undulation accordant with v_ρ, (v_ρ being superficial equatorial velocity, and t_ρ being time of solar rotation).

$v_{'''} = 2f = 2f_0 t^2 = g = \sqrt{2gf} = 2r_0$ = falling velocity communicated, at sun's equatorial surface, by virtual fall through the radius of a circumference equivalent to a primitive wave-length = focal distance of the vertex of a corresponding parabolic wave. Then

$g = \frac{v_0^2}{2F} = \frac{v_{'}^2}{2r} = \frac{v_{''}^2}{2nr} = \frac{v_{'''}^2}{2f}$, and we have the following geometrical series:

$M \propto r^2$	$M_{'} = 2r_0 n^2$	$= 473800\, r_{'}$
$- \propto r^{\frac{5}{2}}$	$- = 2r_0 n^{\frac{3}{2}}$	$= 973{\cdot}3\, r_{'}$
$2r \propto r$	$2r_{'} = 2r_0 n$	$= 2\, r_{'}$
$- \propto r^{\frac{1}{2}}$	$- = 2r_0 n^{\frac{1}{2}}$	$= r_{'} \div 243{\cdot}3$
$v_0 \propto r^0$	$v_0 = 2r_0 n^0$	$= r_{'} \div 118450$

$v_{,} \propto r-\frac{1}{2}$	$v_{,} = 2r_0 n-\frac{1}{2}$	$= r_{,} \div 57640000$
$v_{,,} \propto r-1$	$v_{,,} = 2r_0 n-1$	$= r_{,} \div 28055000000$
$— \propto r-\frac{3}{2}$	$— = 2r_0 n-\frac{3}{2}$	$= r_{,} \div 13654000000000$
$v_{,,,} \propto r-2$	$v_{,,,} = 2r_0 n-2$	$= r_{,} \div 6645600000000000$

The values in the right-hand column are determined by the constant ratio, $n^{\frac{1}{2}}=486{\cdot}7$, which is approximately found by dividing the known value of $v_{,}$ by the approximately known value of $v_{,,}$, as follows:

The approximate values of t_ρ vary between the estimate of Spörer (2127554 sec.), and that of Herschel, Bianchi and Laugier (2188080 sec.). The respective corresponding values of $v_{,,}$ are $\frac{r_{,}}{531888}$ and $\frac{r_{,}}{547020}$ in one second of time. Earth's mean distance from sun being $214{\cdot}86r_{,}$, we find $v_{,}=\frac{2\pi r_{,}\sqrt{2}}{1\text{ yr.}\div 214{\cdot}86^{\frac{3}{2}}}=$ $\cdot 000887r_{,}$ per second; $g=\frac{4\pi r_{,}^{2}}{(1\text{ yr.}\div 214{\cdot}86^{\frac{3}{2}})^{2}}=\cdot 000000393r_{,}$ per second; $n^{\frac{1}{2}}=v_{,}\div v_{,,}=471{\cdot}7$ according to Spörer, $478{\cdot}3$ according to Carrington, $480{\cdot}3$ according to Faye, $485{\cdot}1$ according to Herschel, or, as will be shown below, $486{\cdot}7$ if the velocity of primitive undulation is identical with the velocity of light. Then $v_0=v_{,}n^{\frac{1}{2}}=\cdot 4183r_{,}$ per second (S.), $\cdot 4242r_{,}$ (C.), $\cdot 4295r_{,}$ (F.), $\cdot 4302r_{,}$ (H.), or $\cdot 4316r_{,}$ (v l.).

The value $v_{,,,}=2r_{\rho}$ represents the following comparative considerations of rectilinear or curvilinear motion. Let

v_0 = linear velocity communicated by virtual fall through $\frac{\mathbf{M}}{2}$, = circular velocity acquired by virtual fall through $\left(\frac{r_0}{2}=\text{circum.}\div 4\pi\right)$, in the time of traversing r_0 by revolution in a circular orbit, = parabolic perihelion velocity, or velocity of infinite fall, at a distance of $2r_0$ from the attracting center.

w = a circular wave of same length as the primitive wave, = wave generated by a single centripetal impulse.

$\frac{g}{2}=\frac{w}{4\pi}$ = primary virtual fall corresponding to a single wave-length.

Then $w=4\pi r_{,}\div n^{3}=r_{,}\div 1057700000000000$, if $n^{\frac{1}{2}}=486{\cdot}7$.

If we adopt other estimated values of $n^{\frac{1}{2}}$, the number of wave-lengths per second may be provisionally estimated as between $\cdot 4183r_{,}\div w$ (S.) and $\cdot 4302r_{,}\div w$ (H.), or between $367(10)^{12}$ and $446(10)^{12}$. These extreme estimates both correspond, in rapid-

ity, to undulations near the hottest portion of the solar spectrum,* and indicate *a possible complete identification of the sun's attractive and thermo-dynamic energies.*

According to Struve's constant of aberration, v_λ (the velocity of light) $= r_{,} \div \frac{497{\cdot}83}{214{\cdot}86} = {\cdot}4316 r_{,}$ per second. The corresponding value of $n^{\frac{1}{2}} = 486{\cdot}7$, and the number of vibrations per second $= 457(10)^{12}$, as before stated. Therefore the constant velocity of primitive æthereal impulsion, v_0, which would account for all the gravitating motions of the solar system, *is almost, if not exactly, identical with the velocity of light,* the several approximate values being, in units of light-velocity,

According to Spörer,	·969
" " Carrington,	·983
" " Faye,	·987
" " Herschel,	·997
Light,	1·000

These values exhibit a discrepancy, varying between $\frac{1}{3}$ of one per cent (H.) and 3·1 per cent (S.). If this close accordance is significant of actual physical influence, there are various uncertainties of observation, in the elements of the calculation, which might account for a still greater discrepancy. Perhaps the most important is the uncertainty as to the proper allowance for the differences in the angular velocity of sun-spots in different latitudes. If there were no resistance to the revolution of the spots, their velocity should be planetary, and therefore far greater than it actually is. On the other hand, if they were rigidly imbedded in a rotating mass, their angular velocity should be uniform. The differences are such as to suggest the possibility that the velocity may be partly planetary and partly rotary. In that case it is not unlikely that even Herschel's estimate of v_ρ may be $\frac{1}{3}$ of one per cent too great. The well known thermo-dynamic principles which point to a gaseous structure of the sun, as well as Professor Young's observations on the force of solar explosions, seem to indicate so nice an adjustment of centripetal and centrifugal forces within the sun's mass, as to strengthen the hypothesis that its mean mass, its mass-fluctuations, the varying motions of its spots, and the varying orbital eccentricities of its attendant planets and satellites, may all be continually determined by the constant primitive motion.

The three following additional relationships may, perhaps, prove to be something more than merely curious.

* If the identification is exact, the actual number of oscillations per second is $457\ (10)^{12}$; the number in the extreme red ray being $440\ (10)^{12}$ if we estimate sun's mean distance at 92,000,000 miles.

1. The common ratio of the geometrical series, $n^{\frac{1}{2}}$, is very nearly, if not exactly, $\sqrt{\cdot 4\frac{\text{mass of sun}}{\text{mass of planets}}}$; $r\sqrt{\cdot 4}$ being the radius of spherical gyration.

2. The number of vibrations of the mean caloriferous rays, in the unit of time which satisfies all the equations of the geometrical series, is nearly, if not exactly, the cube of $\frac{2\times\text{sun's mass}}{\text{Jupiter's mass}}$.

3. If the sun's velocity of equatorial rotation be regarded as the velocity at the vertex of a parabolic wave, and consequently as measured by the focal distance of the vertex,* the motion of sun's center in space will be measured by the parameter of the same parabola. This is in weighty accordance with the stellar and planetary correlations which I communicated to the American Philosophical Society† on the 20th Sept., 1872.

If we were to suppose a slight æthereal expansion by the intense heat at the sun's surface, the inrushing colder æther might produce gyrations analogous to our atmospheric cyclones. But independent of any such hypothesis, it is by no means certain that there may not be some slight progressive æthereal motion, combined with the motion of wave-form, or some radial oscillation accompanying the transverse light-undulations. Any such progressive motion, capable of communicating, at each wave-impulse, a velocity $=\frac{v_\lambda}{685\ (10)^{18}}$, would suffice to produce the greatest manifestation of gravitating force in the solar system. In pursuing any investigations for the further elucidation of the subject, due regard should be paid to the double system of centripetal and centrifugal waves originated by inert matter-molecules, and to the æthereal properties which admit of Laplace's supposed instantaneous transmission of gravitating action.‡ It is well also to consider that all the permanent gravitating motions, represented by $v_{,}$, $v_{,,}$ tend to perpetual oscillation, and may therefore be subjected to all the laws of oscillation.

Whatever difficulties we may find in trying fully to account for the foregoing accordances, these accordances are well established *facts* in nature. They are facts in perfect keeping with Newton's original hypothesis, as well as with the hydro-dynamic and mechanical investigations of Challis, Norton, and Leray, with the experiments upon cosmo-plastic forces, and

* $g=2f\therefore\sqrt{2gh}=2\sqrt{fh}$. † Proc. A. P. S., xii, 518–22.

‡ But not necessarily of gravitating *motion*.

with the various harmonies of cosmical and molecular mass and motion which I have published elsewhere; facts which seem to me to lend new probability, if not moral certainty, to the belief in a discoverable and measurable uniform origin of all physical forces.

Philadelphia, August, 1874.

ART. XXXIV.—*On Serpentine Pseudomorphs, and other kinds, from the Tilly Foster Iron Mine, Putnam Co., New York;* by JAMES D. DANA. With plates VI and VII.

THE "Tilly Foster" iron mine is situated about two miles and a half to the northwest of Brewster, on the Harlem Railroad. The rocks of the region are Archæan, being part of the Highland Range, which reaches from New Jersey, across Eastern New York, nearly to the borders of Connecticut. The ore of the mine—magnetite—is distributed, according to a published report, through a band 132 feet wide; and, like that of Northern New York and other Archæan regions, it constitutes a bed conformable to the stratification.

The special mineralogical interest of the locality was first ascertained by Professor O. D. Allen, of the Sheffield Scientific School of Yale College, and the collections made by him have been the source of many of the facts which are here detailed. Prof. Allen has given me further aid in the research by his chemical examinations. Prof. Brush too has kindly placed the specimens in his cabinet before me for study. I have also visited the region and thus added to the number and variety of the specimens under examination. The analyses and descriptions of some of the minerals of the mine by Mr. E. S. Breidenbaugh, in a paper published, in 1873, in the sixth volume of this Journal (p. 207), have given me additional assistance.

I. GEOLOGICAL STRUCTURE OF THE REGION.

1. *Archæan rocks.*—The Archæan rocks of the region are mostly different varieties of syenyte and syenytic gneiss, from black to white in color, and, as usual in Archæan formations, they are often in abrupt alternations, so as to make broad bands, riband-like, of black and white, with black blotchings; and the lines of bedding are much contorted. The syenytic gneiss varies to a granular hornblende rock, containing little feldspar; also to a whitish granulyte-like rock, but little schistose, consisting of quartz and orthoclase, with a very sparse sprinkling of hornblende; also to a hornblendic gneiss, in which both hornblende and biotite (or a black mica) are present: also to a

true gneiss, but only sparingly. The feldspar of the rocks is generally whitish, though sometimes flesh-red; and, judging from the absence of striæ on either cleavage surface, it is orthoclase. Whitish mica (muscovite) is of rare occurrence. Minute zircons may often be found by searching in the syenytic rock with a lens.

The rocks are generally very durable; but at the railroad cut in the village of Brewster, both the syenytic gneiss and the included hornblende rock are crumbling to a depth, in some places, of three or four feet, and this disaggregation appears to be in rapid progress.

2. *Ore-bed.*—The magnetite of the ore bed is more or less mixed with chondrodite. In a portion of it, the magnetite greatly predominates, and the ore passes for massive magnetite. But through the larger part the chondrodite constitutes half or more of the mass, while much of the outer portion of the bed is correctly described as chondrodite containing, along with some other minerals, disseminated grains of magnetite. Massive chondrodite is the chief constituent of the refuse from the mine, and may be had there by the ton.

The chondroditic rock and ore often contain disseminated chlorite; less generally, dark green or greenish-black hornblende and grayish or brownish-gray enstatite; occasionally, disseminated white dolomite and brownish-black biotite; while orthoclase is not found, except in the enclosing syenyte. Molybdenite is occasionally met with, and rarely apatite. A little pyrrhotite and chalcopyrite occur with some of the ore, and still less frequently pyrite.

A small part of the rock is dolomite, with disseminated grains or crystals of chondrodite and occasional grains of magnetite.

3. *Veins in the ore-bed.*—In small veins or nests in the ore-bed, the various minerals occur well crystallized. The chondrodite, chlorite, and magnetite are often in excellent crystals, and with these occasionally apatite; and in some cases cavities were bristled with slender prisms of enstatite. The dolomite is present in simple rhombohedrons, some of them two or three inches across. This mineral usually overlies the other crystals mentioned, but crystals of chondrodite are sometimes isolated in the chlorite and also in crystals of dolomite; and magnetite occurs in these minerals and also in crystals of chondrodite.

Some veins, half an inch to three inches in width, consist mainly of coarsely crystallized chlorite, and others are filled with enstatite in long fibrous masses. Still others consist mainly of dolomite; but this dolomite is a filling, covering beautiful crystallizations of chondrodite, chlorite and magnetite implanted on the walls of the little veins: yet the same dolomite often contains isolated crystals of chondrodite, magnetite or chlorite.

The filling is occasionally dolomite and brucite together; and rarely splendent crystals of chondrodite occur isolated in the latter.

The crystallization of the chondrodite and of other minerals of the bed is now under investigation by Mr. E. S. Dana, and nothing therefore need be added here.

In the wall-rock of the bed there are occasional small veins containing crystals of hornblende and magnetite, and sometimes plates of biotite. The magnetite of these veins is octahedral, with even, polished faces, while that of the ore-bed is dodecahedral, with usually convex and striated faces.

The above species appear to have been crystallized in the fissures they occupied at the time of the crystallization or the metamorphism of the rock. The crystallized chlorite, according to an analysis by Mr. Breidenbaugh (loc. cit.), is the species ripidolite; but it is not certain that all the granular chlorite of the ore bed is of the same species. The color of the ripidolite is generally deep green, rarely reddish.

4. *Minerals of later origin, resulting from alterations of the older minerals, or in other ways.*—Besides these, there is another series of minerals that are manifestly of later origin.

The ore-bed is jointed in various directions. The part containing little chondrodite is mostly solid, with few fractures; but the larger part which contains much chondrodite, along with that which consists mainly of this brittle mineral, is broken throughout to fragments, and so extremely so that the pieces are often smaller than the hand. A great mass of the purer iron ore is in some places in the midst of the more chondroditic; and then the former looks like rock enveloped in a fragmentary deposit arranged more or less concentrically about it. The joints, like those of the Archæan rocks of that vicinity, show that the region has been subjected to disturbing forces; but the extraordinary amount of fracturing is a consequence of the exceeding brittleness of the chondrodite.

The fracturing opened the rocks to movements of water, or moisture in some form, and was the occasion also of such movements and of much chemical action therefrom.

The fragments, large and small, down to those an inch or less in size, are generally coated with a white or greenish *serpentine*, which often looks like a varnish over the surface, and again is an inch or more thick.* They all feel soapy to the fingers on

* Mr. Breidenbaugh found for the composition of the *white serpentine* (this Journ., vi, 209) SiO_2 42·28, Al_2O_3 0·86, FeO 2·57, MgO 40·29, CaO 1·35, K_2O *trace*, Na_2O 0·48, H_2O 12·52=100·35, giving the oxygen ratio for the protoxides silica and water 3·1 : 4·03 : 2, that of serpentine being 3 : 4 : 2.

He obtained for the *biotite*, SiO_2 40·08, Al_2O_3 14·21, Fe_2O_3 11·51, MgO 22·03, Na_2O 0·22, K_2O 9·73, H_2O 1·69, Fl *trace* = 99·47; for the constituents of the *ripidolite*, SiO_2 32·33, Al_2O_3 14·56, FeO 5·29, MgO 33·74, CaO 1·04, K_2O 0·87,

account of it. The great piles of refuse rock that are heaped up near the railroad leading from the mine are, in the main, piles of chondroditic masses thus coated or varnished with serpentine. Over the most of them the serpentine is white, a kind that looks much like meerschaum.

The serpentine also penetrates the masses, and from many of them a fragment of chondrodite as large as a filbert cannot be obtained that has not films of serpentine in or about it. It also fills the cavities in the old veins that were partly filled with crystallizations of chondrodite, chlorite, magnetite and dolomite, so that the crystals of these minerals are buried under it; or it penetrates the veins where there were no distinct cavities.

Besides serpentine, there is sometimes also a coating of *brucite* (hydrate of magnesia), and occasionally this mineral is in large crystallizations. *Fluorite* is another of the secondary incrusting minerals, although not common; it is sometimes in pink massive forms, and occasionally in small amethystine cubes.

In addition, the ore bed abounds in *pseudomorphous minerals*. Crystallizations of chlorite, enstatite, chondrodite, dolomite, apatite and other kinds occur converted into serpentine of various colors. The universal serpentinization of masses and crystals conveys the impression that the rock along all the multitudinous fissures, and, to a large extent, through the interior of solid portions, had been subjected to long digestion in heated magnesian waters.

There are also other kinds of pseudomorphs, indicating great corroding and recomposing power in the waters, as described beyond.

Further, there are species of still later origin. Implanted in the serpentine sometimes occur polished cubes and cubo-pyritohedrons of pyrite; and in seams in the serpentine, the mineral pyrrhotite, another sulphide of iron. There are also, on some of the surfaces of blocks, occasional small groups of crystals of aragonite, or thin crusts of hydromagnesite.

II. The Pseudomorphs and their teachings.

The pseudomorphs which have been thus far observed are of five groups: *first* (A), those consisting of *serpentine*, or of *serpentine and dolomite* combined: *second* (B), those consisting of *bru-*

Na_2O 0·54, H_2O 12·02 = 100·39. Whether all of the chlorite is of the species ripidolite is not ascertained.

The *enstatite* afforded him SiO_2 54·17, Al_2O_3 3·30, FeO 9·94, MnO 0·24, MgO 31·99, CaO 0·99, K_2O 0·16, Na_2O 0·32, ignition 0·13=101·24. He analyzed a massive, faintly fibrous kind. It occurs also long fibrous, and radiated fibrous, the fibers easily separating; G.=3·29.

A brown *chondrodite* gave him SiO_2 35·42, FeO 5·72, MgO 54·22, Fl 9·00 = 104·36; equivalent of oxygen replaced by fluorine, 3·79.

The *dolomite* gave Mr. C. A. Burt (loc. cit., p. 213) CO_2 47·01, FeO 0·70, MnO 0·39, MgO 20·79, CaO 30·14=99·03, making the ratio of carbonate of lime to carbonate of magnesia nearly 1: 1.

cite; *third* (C), those consisting of *magnetite*; *fourth* (D), those consisting of *pyrrhotite*; *fifth* (E), those consisting of *dolomite*.

A. *Consisting of Serpentine.*—Of these there are eleven kinds, 1. Cubic, *after an unknown mineral.* 2. Hexagonal prisms, probably *after calcite.* 3. Hexagonal prisms, probably *after apatite.* 4. Plates, clusters of divergent folia, and masses, *after chlorite* (part, or all, ripidolite). 5. Masses and crystals, *after chondrodite.* 6. Prismatic and massive forms, *after enstatite.* 7. Crystalline massive forms, *after hornblende.* 8. Foliaceous to massive forms, *after biotite.* 9. Rhombohedral, *after dolomite.* 10. Massive, *after brucite.* 11. Rectangular tables or plates, *after an unknown mineral.*

B. *Consisting of Brucite* (hydrate of magnesia).—12. Foliated forms, *after dolomite.*

C. *Consisting of Magnetite.*—13. Rhombohedrons, *after dolomite.* 14. After *chondrodite* and other minerals.

D. *Consisting of Pyrrhotite.*—15. Plates *after serpentine* (that of No. 11).

E. *Consisting of Dolomite.*—16. Pseudomorphs, *after crystals of chondrodite.*

A. Serpentine, or Serpentine-and-Dolomite, Pseudomorphs.

1. *Cubic Pseudomorphs.*

The cubic pseudomorphs—the rarest of those at the mine—often form part of the same specimen with the hexagonal of the second kind above enumerated; and they are shown together in figure 1, plate VI; and fig. 2 is a view of part of the opposite side of the same specimen.

The present form of the specimen is owing to the removal of a portion of the original mass. The bottom layer has usually the cubes with the cubic axis at right angles to the surface beneath; but, above this, the mass conforms mostly to one position, some one of the original crystals having dominated over the rest. This latter fact is well shown in fig. 2.

1. *Composition.*—The pseudomorphs consist either of serpentine, or of serpentine and dolomite combined; and in the latter kind there are all proportions, from those purely serpentine to those that are largely dolomitic.

The serpentine of these pseudomorphs has usually a pale green color, though in some specimens olive-green. According to an analysis by Professor Allen, its composition, after expelling 2·09 per cent of hygroscopic moisture, is

Silica	Magnesia	Water	Fe_2O_3, Al_2O_3
41·87	42·43	13·40	2·30 = 100

and, accordingly, it is the common kind.

The dolomite of these compound pseudomorphs is white and translucent, like ordinary crystalline dolomite. Owing to the

difficulty of separating it entirely from the serpentine, only a qualitative analysis has been made of it by Professor Allen; and this indicated the presence of carbonic acid, magnesia, and lime, and left no reason to doubt its identity with the ordinary dolomite of the mine and of other parts of the specimens.

2. *Structure.*—The structure is the same for the pseudomorphs consisting of both serpentine and dolomite, as for those of serpentine alone. A description of the former, which is of greater interest, will, therefore, suffice for both.

These compound pseudomorphs usually constitute easily-cleavable masses, two or three inches through. The two minerals are united in one crystallized mass, not by intimate mixture, but by side-by-side juxtapositions of independent rectangular blocks or layers of each, all fitted together like parts of a simple crystal.

The *cleavage is cubic* and exceedingly perfect, without the slightest distinction for the two combined minerals, the rectangular blocks (which are always bounded by the cleavage surfaces) separating easily, even more so than those of a crystallized mass of galenite.

Figures 3, 4, plate VI, illustrate this singular tesselated combination, the serpentine being the green portion and the dolomite the white. To appreciate the remarkable delicacy of the tesselation it must be noted that the specimen here figured (taken from the large pseudomorphous mass of fig. 2) is but a third of an inch broad, the view being enlarged five times lineally. In order that the blocking of the serpentine in the dolomite may be better apprehended, the top of the specimen represented in fig. 3 is given separately in fig. 4. Some of the rectangular serpentine spots in these figures are wholly isolated in the dolomite areas; but this isolation is superficial only, for there is internal connection with the other serpentine portions.

Figure 5 affords another illustration of this tesselated structure. It represents a side of a thin plate (a fragment from a large mass) about two-thirds of an inch square and an eighth of an inch thick. The upper, and under, and middle portions are serpentine, and between lie enclosed blocks or layers of dolomite. The lines over the surfaces in the figures represent lines of cleavage, but only those that were externally very distinct.

3. *Origin of the Pseudomorphs.*—This combination of two so distinct minerals, one a hydrous silicate and the other a carbonate, in a common crystalline mass, having one system of cleavage, is proof that, for one or both, the form is pseudomorphous. That both are so, is made manifest by the structure of the minerals.

In the first place, the *serpentine* shows itself to be pseudomorphous by the fact that the cleavage is not *true* cleavage, but

merely a jointed structure; for the smaller blocks afforded by it have no cleavage within, but instead, a wax-like, massive structure; and the uncleavable plates are sometimes quite thick. In true crystals every smallest grain has the cleavage structure as truly as the entire crystal, cleavage belonging to all possible planes in its direction; and, therefore, absence of the cleavage in any part indicates absence of crystalline structure. In some parts of the specimens the cubic cleavage-lines are distinct, *without any cleavage whatever*, the blocks that seemed to be marked out being all solidly united into an uncleavable mass.

It is evident, from the above, that this is not serpentine crystallizing in cubic forms, and, therefore, a new mineral species. It is simply *common serpentine*, which has somehow become possessed of a form foreign to it.

Secondly, the cubic cleavages of the *dolomite* are only joints or divisional planes in rectangular directions. The demonstration of this is found not only in the failure of the cubic cleavage in the interior of the blocks or slices of it, but also *in the existence in the same of the rhombohedral cleavage of ordinary dolomite in all its perfection.* The oblique lines *d*, *d*, *d*, on figure 4 are due to the existence of this cleavage; and in the blocks the cleavage may be obtained indefinitely, precisely as in common crystallized dolomite. Again, portions of one of the masses have sometimes no cleavage but the rhombohedral.

The dolomite is, therefore, *ordinary rhombohedral dolomite.*

Here, then, dolomite and serpentine have together received cubic divisional planes in some way independent of their own powers of crystallization. They have derived it, moreover, from the alteration of one and the same crystallized mineral.

What the original mineral was is not taught us by any thing now occurring at the iron mine. It must have had, not only easy rectangular cleavages, but an open cleavage structure, that is, cleavage-joints, such as exist in galenite; for these cleavage-joints are the divisional planes or joints that are retained in the pseudomorph.

As crystallized galenite (sulphide of lead) has cubic cleavages and also cleavage-joints, it is a question whether this was not the original mineral in the case of this pseudomorph. But the change of a sulphide of lead to dolomite or serpentine, or its removal and the concurrent substitution of these minerals, is hardly supposable.

Anhydrite (anhydrous sulphate of lime), although orthorhombic, has easy rectangular cleavages in three directions and its masses have generally rectangular fractures throughout, following planes of cleavage; so that it possibly might sometimes give cubic jointing to its pseudomorphs. Moreover, the mineral is occasionally found in veins of ore, as in the Hartz and

at Fahlun in Sweden. But the crystalline masses of this orthorhombic mineral are made up usually of tabular cleavable masses, and never have the perfect cubic regularity of divisional planes found in these pseudomorphs.

Common salt has at times the cubically-jointed structure required for producing such pseudomorphs. But its presence in an iron ore bed is improbable; and, if present, it could hardly undergo a change to dolomite or serpentine and retain its cleavage structure, on account of its easy solubility.

Fluorite (fluor spar) occurs in masses made up of cubes; but strictly and only cubes; and its cleavage is octahedral.

I can make no other suggestions as to the original mineral. This much is certain, that the species was isometric in crystallization, and had easy cubic cleavage; and its crystallized masses had numerous cleavage-joints.

Whatever the mineral, it underwent two kinds of pseudomorphic changes. We note, *first*, that the two changes could not have gone on together: for two so different minerals could not have been simultaneously formed in the same crystalline mass from the same chemical solution. *Secondly*, that the dolomite must have been formed before the serpentine portion; for the serpentine blocks or plates have the faces perfectly smooth and polished, while those of dolomite, while very even, always appear faintly eroded, as seen under a lens.

Was *part* of the *original* crystalline mass first changed to dolomite, and the rest subsequently to serpentine? Or, was the whole mass first changed to dolomite, and afterward a part of it changed to serpentine?

The last supposition seems to be the most reasonable. The change from the original cubical mineral to dolomite must have gone forward through infiltration along the open cleavage-joints, and thus these cubic cleavage planes were imparted to it. The change of part of the dolomite (or of the whole in some cases) to serpentine took place subsequently, by the same general method, and so the dolomite gave the latter its borrowed cubic cleavage-joints.

Further, this change of the dolomite took place by rectangular blocks, one such rectangular plate or block being changed alike throughout, when another adjoining, separated only by planes of the cubic cleavage, remained unaffected. This pseudomorphism by blocks seems at first improbable; yet the specimens prove it to be a fact, and other examples of it are given beyond, showing that it is a common method.

The question comes up: Could the chemical change from dolomite to serpentine have gone forward through an alkaline solution of silica, the magnesia having been derived

from the dolomite; or through a solution of a magnesia-silicate, that is, of serpentine itself? The former supposition can not be true. For if a block of dolomite had been changed to serpentine in that way, it would have been changed also in density, and therefore in size; and the difference in size would have been apparent by displacements throughout the pseudomorphous mass. The fact is, all parts are fitted together as exactly as if the whole were of one mineral. It follows then, if the serpentine has displaced dolomite (instead of the original cubic mineral), that the material introduced in solution to effect the change was not silica, but a magnesia-silicate. It follows also that the same magnesia waters had the power of dissolving, and so removing the dolomite; and that the infiltering magnesia-silicate took the place of the dolomite as the removal went on; it thus being a case of substitution and not of alteration.

If the *first* of the above suppositions is true, there was no change of dolomite to serpentine, but only of the original cubic mineral to each dolomite and serpentine—part to one, and then the rest to the other; and both must have been cases of substitution or removal, in order that the blocks should fit together with the exactness characterizing the pseudomorphs.

2. *Hexagonal prisms, probably after Calcite.*

The hexagonal prisms here referred to are terminated in three rough rhombohedral planes (fig. 1, plate VI); and, as nearly as I can ascertain, the angle between the latter planes is that of calcite. The prisms, several of which occur together in groups, are half an inch to two-thirds in diameter, and one of those examined is two inches in length. They consist mainly of serpentine, but contain a little dolomite. These large prisms have a thick coating of serpentine, which externally is smoothly rounded and shining. In figure 1, two of the prisms of a group are shown partly denuded of the coating, while a third has still the coat on. This coat is a tenth to a sixth of an inch through, and is transversely semi-columnar in structure.

Beneath this coating there is usually some incrusting white dolomite, granular-crystalline in structure; and the base of one crystal is mainly dolomite, with some small pieces of magnetite.

The original mineral of these prisms was probably calcite (carbonate of calcium), as the form is a common one of that species. It is possible that dolomite may occur under such a form; but so large and long prisms of dolomite trihedrally terminated have never been observed. Since the dolomite that is now in the pseudomorphs is an aggregation of crystalline grains instead of having the cleavage of a simple crystal, it is not part of the original mineral.

3. *Hexagonal prisms, probably after Apatite.*

These prisms occur imbedded in the cubic serpentine. They are slender, being less than a line in diameter. By measurement, they are regular hexagonal prisms; and as apatite is one of the occurring minerals of the mine, they are probably pseudomorphs after that species.

Relations of the Cubic to the Hexagonal Pseudomorphs, and to the associated Minerals.

(1.) In the specimen, a portion of which is represented in fig. 1, the cubical portion *rests upon* the coating of the rhombohedral portion; and over a large part of the specimen the former is easily separated, and leaves exposed the shining surface of the latter. Only in a small part are the two soldered together. The prismatic pseudomorph in view has been exposed by such a removal.

It is thus demonstrated that the group of large hexagonal prisms preceded the existence of the cubical portion. We hence discover that the following was the order of events.

1. The crystallization of a group of hexagonal prisms, probably of calcite.

2. The change of these crystals to serpentine and partly to dolomite.

3. The incrusting of these prisms, after this change, by serpentine, making a coating over the whole, as shown in figure 1.

4. The deposition, over this coating, of a cubic mineral whose masses had many cubic cleavage-joints.

5. The change of the cubic mineral to dolomite, by infiltration along the open cleavage joints, and hence with a retention of many cubic cleavage surfaces.

6. The change of part of the dolomite, thus cubic in cleavage surfaces, to serpentine, through the infiltration along the cleavage-joints of a solution of magnesia-silicate, the alteration affecting entire rectangular blocks or plates of the dolomite, but leaving throughout adjoining blocks or plates unaffected.

While proving here that there were an earlier and a later era of pseudomorphism, it is not proved that the successive eras may not have been comprised within a single epoch of pseudomorphism. They may have been successive events in a single month or year.

(2.) With regard to the relations of the pseudomorphs to the associated minerals, we note:

In an olive-green specimen of the cubic serpentine there are imbedded tabular crystals of chlorite (ripidolite), and also large dodecahedral crystals of magnetite. Along with these minerals occur the pseudomorphs after apatite.

This association proves either that the crystals of chlorite and magnetite were formed at the time the original cubic min-

eral was deposited: or that they were made during the change of the cubic mineral to serpentine. The former may have been the fact; and yet there is no trace of alteration in the chlorite crystals to show that they have passed through a time of serpentine deposition.

4. *Pseudomorphs after Chlorite.*

The change of the crystallizations of ripidolite to serpentine, in specimens from this locality, was early observed, in all its stages, by Professor Allen. Mr. Breidenbaugh, in his account of the white serpentine, remarks that there is a gradual shading in the color of the ripidolite from bright green to pure white, and "in its texture from the foliation and transparency of the unchanged mineral to the compactness and opacity of the serpentine." Specimens are common. Some are crystals, white and pearly, retaining the form of the ripidolite; others, aggregations of whitish folia, from fissures half an inch to three inches in width; others, white, grayish or greenish divergent fibrous masses, either large divergent groups, or stellated aggregations; and others are structureless white or greenish serpentine.

Many of the specimens illustrate the progress of the change from chlorite to serpentine. The surface of a half-changed crystal is often marked with green and white, as represented in figures 6 and 7, plate VI; showing that, in the change, the cleavage-joints were a barrier to its progress; portions of the chlorite bounded by cleavage lines remaining still green, while other portions outside and beneath are wholly changed, on the principle illustrated in the cubic pseudomorphs. The green plates in the figure have the angles 60° and 120°, and perfectly even sides.

When the change to serpentine is complete, there is often one or more of the outer folia on one side or another of the mass that still has some of the color of the chlorite and removes all doubt as to the origin of the foliated mass. But from these there are gradations to other varieties, in which the foliaceous or radiated structure is wholly lost.

The massive crystalline-granular chlorite of the ore-bed also occurs changed to serpentine; some of it retaining the granular structure, and other portions destitute of it. The color is often dark olive-green, while that of the crystals and foliated masses is white or pale green. This fact suggests that this massive chlorite may be another species containing more iron. Breidenbaugh found in a massive chlorite of the mine 9·62 per cent of protoxide of iron, while the ripidolite crystal afforded only 5·29 per cent; but his analysis leaves some doubt as to the nature of the former species.

(To be continued.)

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the First Products of the Distillation of Benzol.*—It is well known that in the rectification of benzol, a considerable portion of distillate comes over at temperatures below that at which benzol itself boils. Helbing has examined, in the laboratory of Professor Erlenmeyer, a sample of this first distillate obtained from J. Weiler & Co., Cologne. It was a yellow, disagreeably-smelling liquid of specific gravity 0·899 at 17·5°, becoming turbid on strong cooling. Elementary analysis gave carbon 80·07, hydrogen 6·71, sulphur 13·61 per cent, thus excluding metacetone and all other oxygenated products. By careful fractionating in Linneman's apparatus, for an entire month, no liquids of absolutely constant boiling point could be obtained. The fractions obtained between 35° and 60° gave readily, when treated with alcoholic potash, potassium ethyldisulphocarbonate; thus proving the presence of carbon disulphide. The fractions between 60° and 75° also afforded CS_2. Bromine acted energetically on all the distillates. Finally, the largest portion of the liquid collected into products boiling between 43° and 55°, and above 75°. On cooling the distillate obtained above 79°, it solidified completely; that from 75° to 79° only partially. By repeating the cooling process, 460 grams benzol were obtained from 900 of the crude product; or about 51 per cent. Calculated from the sulphur in the elementary analysis, the amount of carbon disulphide must be about 16·28 per cent. The fractions which had come over between 18° and 40°, were then subjected to fractional distillation and fractional condensation; but in neither case could a product be obtained free from CS_2. Treatment with alcoholic potash being no more successful, the author gave up the attempt to isolate the hydrocarbons as such, and treated them with bromine, in order to convert them into bromides. Owing to the violence of the action this required great care; and the product, after washing with dilute potassium hydrate solution and with distilled water, was a heavy yellowish-red liquid of disagreeable odor. On distilling it in a current of steam a colorless strongly-refracting liquid first came over, which was nearly pure CS_2; then followed a heavy, slightly yellow liquid of an agreeable sweetish odor; and finally a few oily drops collected in the receiver, which solidified on cooling to a yellowish-white fatty mass. On analysis the fluid bromide just mentioned afforded numbers agreeing with the formula $C_5H_{10}Br_2$, or amylene bromide. The solid bromide gave on analysis the formula $C_4H_6Br_4$, or that of crotonylene tetrabromide. The fraction of the crude product boiling between 50° and 70° was likewise converted into bromide, fractionated and analyzed. It gave the formula of hexylene bromide. Helbing concludes therefore that the first runnings of the benzol still contain considerable amylene, less crotonylene and still less hexylene.—*Ann. Chem. Pharm.*, clxxii, 181, July, 1874. G. F. B.

2. *On the Preparation of Active Amyl alcohol.*—In studying the cause of the anomaly presented by amyl chloride of rotating to the left, as distinguished from the iodide and bromide, which rotate to the right, LE BEL found that the amyl chloride obtained by saturating the crude product of the distillation of amyl alcohol with aqueous hydrochloric acid, by hydrochloric acid gas and again rectifying, was optically inactive. Assuming that this result was due to the presence of yet undecomposed alcohol in the chloride, it was treated, one portion with sulphuric acid to unite with and so to retain this alcohol, and another portion with phosphoric chloride to convert this remaining alcohol into chloride. Upon subsequent distillation, the two products obtained were both right-handed, but the latter was much the more active. Hence the last products of the conversion of amyl alcohol into chloride are much the more optically active; as also must be the alcohol from which they are derived. To prove this, a portion of amyl alcohol rotating 1° 58 to the left was treated fractionally with hydrochloric acid gas, until nine-tenths of it had been removed as chloride. The remaining tenth rotated 4° 32′, being 32′ more than the alcohol obtained by Pasteur. The boiling point of this alcohol was 127°, and that of its chloride 98°. The author attributes the very different rotatory powers ascribed to the amyl compound ethers, to the presence of these two alcohols in the original substance used, with one of which certain acids unite more readily than with the other.—*Bull. Soc. Chim.*, II, xxi, 542, June, 1874. G. F. B.

3 *On a new method of preparation of Salicylic Acid.*—Wishing to investigate more thoroughly the character of salylic acid, asserted by him several years ago to be an isomer of benzoic acid and therefore of great importance in chemical theory, KOLBE was led to examine more fully the synthetic modes of producing salicylic acid, from which it is derived. His attention was at first directed to the method described long ago by himself and Lautemann; to act upon phenol with carbon dioxide in presence of sodium. Observing that when sodium was dissolved in hot phenol, a stream of dry carbon dioxide gas being passed through the apparatus, there is formed beside sodium salicylate, also sodium carbonate and sodium phenylate, and that the more of the former, the less of the latter product was formed; and further that the sodium phenylate on more strongly heating it, absorbed more carbonic gas and yielded sodium salicylate, Kolbe was led at once to propose a new and simpler method of preparing salicylic acid; a method too, applicable on the large scale.* In strong commercial soda lye of known strength, so much melted phenol as is necessary to saturate it is dissolved. The whole is then evaporated in a shallow iron vessel, with constant stirring and crushing, until a dry powder is obtained. This powder, which is sodium phenylate, is then heated in an oil or air-bath, best in a metallic retort, to 100° and dry carbonic gas

* The manufacture of salicylic acid on a commercial scale by this process has been undertaken by Dr. von Heyden in Dresden. Kolbe refers chemists desiring to work with this acid to this fact, and asks the exclusive privilege for a year from this fall of working upon salicylic and paraoxybenzoic acids in this connection.

is passed over it. The temperature rises to 180°, and then phenol begins to distil over. The process is finished when the temperature has reached 220° to 250° and no more phenol comes off. The contents of the retort when cool are dissolved in water, acidified with hydrochloric acid, heated to expel phenol, filtered and allowed to cool. Salicylic acid, in nearly the theoretical quantity, crystallizes out. The same result is afforded by calcium and barium phenylates; but the very curious result was observed by Kolbe that potassium phenylate gave paraoxybenzoic acid when thus treated; due apparently to the temperature.—*J. pr. Ch.*, II, x, 89, July, 1874. G. F. B.

4. *Vanilline.*—At a meeting of the Paris Academy of Sciences held Sept. 14, Dr. W. A. Hofmann announced that his two students, MM Tiemann and Haarmann, who had obtained vanilline (the aromatic principle of the vanilla bean) from pine sap, propose to manufacture this substance on a large scale. The sap of a tree of medium height gives vanilline to the value of 100 fr., and the wood is not injured by the extraction of the sap. This will be the second vegetable product manufactured by purely chemical methods.—*Nature*, Sept. 24.

5. *Expansion of Hard Rubber.*—M. Kohlrausch having several times noticed that glass flasks, closed by stoppers of hard rubber, burst, concluded that this substance must be very dilatable. This hypothesis was fully verified by experiment, for the expansion of this body was found to be about three times that of zinc. From his measures, the coefficient of dilatation for 1° between 16°·7 and 25°·3 = ·0000770, and between 25°·3 and 35°·4 = ·0000842. Thus, not only has hard rubber a very great coefficient of dilatation, but the latter increases very rapidly with the temperature.

This remarkable property can be applied to the construction of very delicate thermometers. Thus, with a small instrument, consisting of two strips of rubber and ivory, 20 cms. long, glued together and fastened at one end, we obtain, at the other extremity, a movement of several millimeters for a change of temperature of one degree. The coefficient of hard rubber is equal, at zero, to that of mercury; above, it is greater. We can, then, as a curiosity, construct a mercury thermometer with a reservoir of this substance, whose changes will be the opposite of those of a common thermometer, and which will fall with an increase of temperature.—*Bib. Univ.*, cxcix, 311; *Pogg. Ann.*, cxlix, 577. E. C. P.

6. *Air Pressure required to sound various Wind Instruments.*—Dr. W. H. Stone, in a paper before the Physical Society of London, describes some experiments on the wind pressure in the human lungs during performance on wind instruments. The first experiment aimed simply at measuring, by a water gauge, the extreme pressure which could be supported by the muscles of the lips, both of trained musicians and others. About six feet of water was the ordinary maximum when a small tube was inserted between the lips. When the lips were supported by a capped mouthpiece, as in brass instruments, a much greater pressure could

be sustained, and the lip muscles invariably gave way long before the expiratory power of the thoracic muscles was exhausted.

A second experiment consisted in introducing a small bent tube into the angle of the mouth, connected with a flexible tube passing over the shoulder. It was found that most instruments could be played as well with this addition as without it. It obviously established a communication between the cavity of the performer's mouth, and therefore of his thorax, and the pressure gauge. The following table was compiled from many observations on some of our principal English musicians. The small tube was inserted in his mouth, and he was directed to sound in succession the chief notes of his instrument. As soon as the tone became full and steady, the position of the water gauge was noted. A fair "mezzo-forte" note was employed. Of course, by forcing the wind and overblowing the instrument, a much greater pressure could be obtained, but those given here were sufficient to produce an average orchestral tone:

Oboe,	lower notes,	9	inches,	highest,	17	inches.
Clarinet,	"	15	"	"	8	"
Bassoon,	"	12	"	"	24	"
Horn,	"	5	"	"	27	"
Cornet,	"	10	"	"	34	"
Trumpet,	"	12	"	"	33	"
Euphonium,	"	3	"	"	40	"
Bombardon,	"	3	"	"	36	"

It will be noticed that the clarinet in this, as in some other respects, differs from its kindred instruments, and also that most of the pressures are small, not exceeding, or, indeed, attaining, the pressure of a fit of sneezing or of coughing. They are, therefore, very unlikely to injure the lungs, or to produce the emphysema erroneously attributed to them.—*Phil. Mag.*, xlviii, 114. E. C. P.

7. *Forces caused by Evaporation from a Surface.*—Prof. O. Reynolds, in a paper before the Royal Society, gives a new explanation of the observations of Prof. Crookes and others, that, under certain circumstances, hot bodies appear to repel, and cold ones to attract, other bodies. Prof. Reynolds claims that these effects are the results of evaporation and condensation, and that they are valuable evidence of the truth of the kinetic theory of gases, viz., that gas consists of separate molecules, moving at great velocities.

A light stem of glass with pith-balls on its ends was suspended by a silk thread in a glass flask, so that the balls were nearly on a level. Some water was then put in the flask, and boiled until all the air was driven out of the flask, which was then corked and allowed to cool. Approaching the flame of a lamp to one of the pith-balls, it was repelled, while a piece of ice attracted the ball. If more than a very minute amount of air was present in the flask, these effects were masked by the formation of convection currents. When a piece of ice was held near one of the pith-balls, it was attracted, but after a short time the moisture condensed on it,

rendering it heavier than the other ball. The wet ball was then more than ordinarily sensitive, while the other, which had become dry, was nearly insensible to heat. On removing the air, and so much of the vapor that the pressure was less than that due to the temperature, the balls became dry, and were no longer sensitive to the lamp, although still affected by the ice. When no vapor was present, the convection currents reigned supreme, even with very small pressures.

These experiments seem to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward. According to the kinetic theory of gases, during evaporation the particles are shot off from the surface, and, since action and reaction are equal, they will produce a corresponding pressure back on the surface, corresponding to the force producing the recoil of a gun. In the same way, during condensation, the particles are stopped as by a target, and produce the effect of a pressure. Each of these impulses will, however, be less than that due the ordinarily impact of a particle, since the particle is, during condensation, simply stopped, while ordinarily it is first stopped and then thrown with equal velocity in the opposite direction. The pressure is, therefore, increased during evaporation, and diminished by condensation. In the case of water at 60° the evaporation of 1 lb. would be sufficient to maintain a force of 65 lbs. for one second. In the case of mercury, this force will be only 6 lbs., but the latent heat of mercury being only one-thirtieth that of water, the same expenditure of heat would maintain nearly three times as great a force as in the case of water. In Prof. Crooke's experiments the use of the Sprengel pump to produce a vacuum seems to account for the presence of a condensable vapor.—*Phil. Mag.*, xlviii, 146. E. C. P.

8. *Index of Refraction of Liquids.*—MM. Terquem and Trannin propose a new method of determining the index of refraction of liquids, based on total reflection. A small tank with parallel sides of glass is used to contain the liquid. In this are placed two plates of glass cemented along the edges and free to turn through a measurable angle. The whole is placed between the collimator and observing telescope of an optical circle. The image of the slit being distinctly seen, the plates are turned until total reflection takes place, when the slit disappears. Then turning the plates in the opposite direction, a second measurement is obtained, and the difference equals twice the limiting angle of the liquid with regard to air. With common light the image turns red before disappearing, but with monochromatic light the disappearance is almost instantaneous and can be determined within a quarter of a minute. With a Geissler tube containing hydrogen as a source of light, the image undergoes two marked changes of color, due to the total reflection of the two rays $H\gamma$ and $H\beta$. The error then is only about half a minute, and for $H\alpha$ about a quarter of a minute. The measurements of several liquids are given, agreeing very closely

with previous determinations, the difference being readily accounted for by the difficulty of obtaining the liquid perfectly pure. —*Comptes Rendus*, lxviii, p. 1843. E. C. P.

9. *Electrical Phenomena.*—P. Dom. Marianini describes two curious phenomena illustrating the difference in the spark from a body electrified positively or negatively. Two metallic forks have two prongs, one terminating in a point, the other in a ball, and placed opposite each other, so that each point is opposite a ball. An electric spark will pass through one or the other, according as it is positive or negative.

The shorter end of the siphon dips in a vessel of water, and the larger end is drawn out to a point, so that, owing to capillarity, no water will flow. The water level may then be raised or lowered by trial until it reaches such a height that the siphon throws out diverging drops when connected with a positive source of electricity, and there is no flow when the source is negative. The opposite result takes place when the end of the siphon is placed opposite the machine, the water being connected with the earth.

M. A. Roiti attempts to determine whether the electric current is an ether current, by the fact that the velocity of light in a liquid traversed by a current ought to be influenced by the direction of the current, if their velocities are comparable. He employs an arrangement like the interferential refractometer of Arago, and finds that there is no sensible action.—*Il nuovo Cimento*, ix, 97, 148; *Journ. des Phys.*, iii, 227. E. C. P.

10. *Note on the view of Mallet as to the Fusion of Metals;* by Adolf Schmidt, of the Missouri Geological Survey. (Communicated.)—This Journal for September mentions a paper read by Mr. R. Mallet, before the Royal Society, on the fusion of metals. In that paper the author is said to have explained the fact that some metals, when solid, float on a melted bath of the same metal, by the assumption of a "repellent force." Before definitively adopting this rather mysterious explanation, I recommend to all who are yet in doubt in regard to this subject to make the following experiment:

Have a solid ball of cast-iron of 1½ to 2 inches diameter cast and filed off pretty smoothly. Have a ladle or vessel of at least ¾ cubic foot capacity filled with molten cast-iron. If then you lay the cold cast-iron ball on the surface of the molten iron, you will find that the ball, in spite of the "repellent force," assumed by Mr. Mallet, will sink to the bottom of the ladle at once. With an iron rod you can feel the ball at the bottom of the ladle and roll it about. But, after twenty or thirty seconds, the ball will slowly rise to the surface of the bath and remain there. It is thus evident that cast-iron at ordinary temperatures is both *heavier* and *denser* than molten iron, but that, as its temperature rises, the solid iron expands, and becomes lighter and finally floats on the molten iron. The latter fact shows simply that solid iron, when at a high temperature, approaching its melting point, is less dense and lighter than molten iron, which fact again implies that

molten iron must undergo a rapid expansion in the moment of its solidification. The extent of this expansion is, however, less than that of the subsequent contraction in cooling, so that the cold iron is again denser than the molten iron.

The error of Mr. Mallet and of many preceding observers consists in this: Their observation, that the solid metal floats on the molten metal refers to the former *when heated*, while their determinations of specific gravity of the solid metal are made with the metal *when cold*. But my experiment, as above described, shows that this cold metal, which has the highest specific gravity, does *not* float, and the heated metal which *does* float has undoubtedly a smaller specific gravity. There is certainly nothing either incongruous or wonderful in all this, and nothing that would require or justify the assumption of a "repellent force." None of Mr. Mallet's experiments, as far as they are mentioned in the "Journal," prove anything against the temporary expansion of certain metals in the moment of solidification, and all the observations I made on this point in foundries verify it.

Washington University, St. Louis, Mo., September 21st, 1874.

II. Geology and Natural History.

1. *Notes on the Geology of Costa Rica;* by W. M. Gabb.—[The following letter was recently received from Mr. Gabb, who has been at work exploring in Costa Rica during the year past. His labors are bringing much that is new to light over a region of which little is geologically known. We trust that they may be continued until the resources, features, and structure of the country are thoroughly investigated. No one is better fitted than Mr. Gabb for the work.—J. D. D.]

San José, Costa Rica, Aug. 10, 1874.

I have just returned from my last trip into the wilderness of Talamanca, and am now winding up the work in the shape of reports, maps, etc. The close of the survey, in fact the last half, found me the only representative of the original corps. Not only all of my first assistants, but in some cases two relays of substitutes, gave out and retired, with health seriously injured. One man, now a year in a healthy climate and in the doctor's hands, is not well yet. Fortunately, I lost no lives, and, so far as I myself am concerned, neither my life nor my spleen is injured, and except being a little lighter in weight I am as good as new, and ready to go into the field again as soon as my report is finished and *my money paid!*

We were about four months away on my last journey, and while our hardships were neither few nor trivial, our scientific results were satisfactory.

We reached the summit of Pico Blanco June 13, and spent three hours on the summit. Without having at hand the tables for going into the calculations for corrections, etc., the barometric results are approximately 10,200 feet. This is 1,500 feet lower than

the formerly received opinion. Where this originated I cannot learn. From the flank of Pico Blanco I made a rude leveling across to the summit of the "U-jum," mentioned in my last letter, and got its approximate height—about 9,600 feet. The other one, mentioned on the strength of reports of the Indians, and which I had not seen, is 200 to 300 feet lower.

I find that "U-jum" means in the native language a bold peak, and is a generic term. It is applied by the people equally to the summit of "Kamuk" (Pico Blanco). For distinction, therefore, I propose to retain for the last named its Spanish name; for that at the head of the Coer we might accept the word U-jum, more especially since I have already used it in a specific sense; while for the intermediate peak, at the head of the ridge between the Lari and its tributary, the Dipari, I suggest the name of Mt. Lyon, in honor of my friend Mr. J. H. Lyon, the only white man in Talamanca, and without whose active sympathy and unremitting exertions our expedition could not but have been a failure.

To estimate Pico Blanco, I give you the data,

Old Harbor, sea level,	barometer,	39·042,	att. therm.	80,	det. therm.	81
Summit of Peak,	"	19·814,	" "	63·5,	" "	62

Its geography has been misstated by some writers, who have placed it on a spur of the Cordillera. It is distinctly in the center line of the main chain, the waters falling rapidly from it toward the two oceans. In this connection we have robbed Iraza, the "show mountain" of Costa Rica, of one of its chief glories. Everybody who comes to the country rides up to the crater on mule back and then writes a book about his achievement, not omitting to state that this is the only point in the world where one can see both oceans at once.

From even as low as 600 feet below the summit of Blanco we saw at a glance thirty miles of the Atlantic, and all of forty of the Pacific. This is a little better than a glimpse of near Greytown on one side, and a suspicion of the Gulf of Nicoyo on the other, which the Iraza (or volcano of Cartago) people get.

Geologically, Pico Blanco must henceforward be erased from the list of volcanos. It is the culminating point of a granite intrusion from below Miocene rocks. I say intrusion, after due weighing of probabilities as to Azoic core, which I know will suggest itself to you. I have not space here to enter into details. Only one fact can be dwelt on now. Not a pebble of granite or syenite has yet been found by me in our conglomerates!

While I say it is *not* a volcano, yet there is a large mass of true volcanic rock forming the apex. It is, however, only a dike, laid bare by denudation, and does not extend 300 feet below the summit. Hundreds of similar dikes are found all through the hills, but have to be looked for, surrounding circumstances not having been so favorable for bringing them into view. Besides, there is not the slightest trace of a crater form, either entire or in part. The top is a straight, ragged ridge, with an isolated point at the west end, a trifle higher than the other part.

I do not know what to say of U-jum and Mt. Lyon. Like Blanco, the last few hundred feet of their summits are bare of forest. On the mountain visited this is replaced by a growth of sage, furze, heather, moss, fern, whortleberries of a light reddish brown color, etc. The fires, smoke, etc., reported at times may result from the burning of this vegetation, but they certainly do *look* volcanic at a few miles distance.

One day, when the sky overhead was clear, I heard thunder from a distant storm; at least, so I believe it to have been. But two or three of my Indians exclaimed at once "*U-jum oruna*" (U-jum is angry). There is no active volcano in their country or within their ken, if U-jum is not one, and how could they know of volcanic rumblings except by some such experience? I must hold my opinion in abeyance until good fortune enables me to visit one or both of these interesting peaks.

But my sheet is nearly full, and I have left much unsaid. We have now full notes for a thorough topographical map of the region. I have all of the geology and large collections in zoology, and more than anybody else will probably ever get in ethnology.

2. *Note on the occurrence of Metamorphic Silurian Rocks in North Carolina;* by Prof. FRANK H. BRADLEY. (From a letter to J. D. Dana, dated Knoxville, Tenn., September 15th, 1874.)—In my recent trip into North Carolina, from which I have just returned, I determined the metamorphic rocks of the southwestern corner of that State and of the adjoining part of Georgia to be unquestionably of *Silurian* age. I did not go east of Franklin; but, so far, they are all *Lower* Silurian. The crystalline marbles of Murphy and vicinity—white, black and flesh-colored—which are partly siliceous and inclose a rich bed (?) of gold-bearing quartz, are the precise equivalent of our Knox limestones of Quebec Group age, though much thinner than the equivalent beds in East Tennessee. Like their unaltered equivalents in the Great Valley, they are accompanied by heavy beds of brown hematite of the best quality. They are accompanied by heavy beds of agalmatolite and itacolumite, and as these rocks are said to also accompany the limestones of Gainesville, Ga., on the south side of the Blue Ridge, I suspect that these latter will prove to be of the same age.

3. *Abstract of a paper on the Trap Rocks of the Connecticut Valley;* by E. S. DANA. (Read before the American Association at the Hartford Meeting, Aug. 1874.)—This paper was a report of some preliminary results obtained in a series of investigations now being carried on by E. S. Dana and G. W. Hawes.

The trap belongs to an extensive series of fissure eruptions connected with the Mesozoic sandstone, not only of the Connecticut Valley but also of Nova Scotia, New Jersey, Pennsylvania, and North Carolina. These dikes of trap, however, are nowhere displayed to such extent and in such numbers as in our vicinity—in Connecticut—where they have been studied topographically and mapped with a marvelous degree of accuracy and minuteness by Percival.

The trap rock, microscopically investigated, shows throughout a crystalline texture. It is made up of pyroxene, labradorite and magnetite, with also occasionally some chrysolite and apatite. Chlorite is often present as the result of local change. The pyroxene sometimes, in coarse varieties, runs off into long-bladed prisms, somewhat resembling hornblende, which name has in consequence often been given to it. The pyroxene is the first constituent to suffer from surface alteration. The magnetite is commonly in irregular masses, but sometimes shows curious and beautiful arborescent crystallized forms, frequently observed elsewhere in similar rocks. It is interesting to observe that these peculiar dendritic forms are confined, as far as now observed, to the more hydrous of the trap rocks, although future study may not confirm this. The feldspar shows before the microscope its triclinic character; and an analysis of the rock by Mr. Hawes proves that it has the composition of labradorite. This analysis gives for the rock exactly the composition of a dolerite, and it must receive this name, as it has generally done hitherto, though in this we go counter to foreign usage, according to which dolerite is a younger rock, not older than the Tertiary. It is important to observe that the rock, as it contains no hornblende, is not diorite, though that name has also been given to it.

Turning, however, from what may be called the normal rock, for example, that analyzed by Mr. Hawes just referred to, containing almost no water, we find other varieties containing a considerable amount; and here the microscope comes to our assistance, making it possible to extend our observations over a wide range in a short time.

We find that the trap which has come up *through the older crystalline rocks* is most of all free from hydrous minerals, or any evidence of alteration, its grains having a fresh, vitreous look on the fracture. This is also true, but not quite so completely, of trap from the West Rock range, which adjoins the region of crystalline rocks, and that from East Rock near New Haven, a little more to the eastward, and of trap from other points south and west. As we go from West Rock toward the eastern side of the Mesozoic sandstone region, the character of the rock changes; it loses its lustre and hardness, and is often amygdaloidal. The change is due to the hydrous character. The rock from the Saltonstall ridge, for example, contains 4 to 5 per cent of water. This trap is throughout more or less green under the microscope, containing a chlorite, sometimes in plates and seemingly made at the expense of the pyroxene, sometimes in cavities, these last often entirely invisible to the unaided eye. This general character belongs to the trap more or less decidedly from East Haven north, following the eastern range through the Durham or Middletown Mountain; it is also true of a large portion of the Meriden range and its continuation north to Mt. Tom.

In addition to this massive, though generally chloritic trap, which makes up most of the great ridges laid down on Percival's

map, we find also another variety quite different; this is light green in color, soft, very hydrous, and has its feldspar as well as pyroxene very much altered. It is most characterized by its amygdaloidal structure, the cavities being very numerous, filled with calcite or chlorite or quartz, and sometimes with datolite or anaclite. The cavities are also sometimes in part filled, curiously enough, with bitumen; this is true of some dikes west of Hartford. This amygdaloid, moreover, is found in a series of low, subordinate ridges parallel to or concentric with some of the most prominent ranges, for example at the Meriden Hills, as first pointed out by Percival—one of the most curious phenomena connected with this subject, and not easy of explanation.

The fact, just alluded to, that the amygdaloidal and massive trap occur in the same dike, seems to shut out the idea that these subordinate ridges are different in age from those adjoining. If we accept the idea suggested by Prof. Dana in a recent article, that the moisture in igneous rocks found access to them while they were in process of eruption, we may regard all these as results of changes wrought in what originally was essentially the same material by local causes in introducing moisture. In accordance with this view the trap intersecting the crystalline rocks is anhydrous, while that of the interior of the sandstone region, where alone subterranean streams of waters were possible, are more or less hydrous. But I leave the subject here, since until many more facts have been collected and the work of the microscope has been supplemented by that of the laboratory under the hands of Mr. Hawes, any definite conclusion will be unsafe.

In a few individual cases the Triassic trap from Nova Scotia, New Jersey, Pennsylvania, and North Carolina has been examined, and as far as microscopic structure goes the rock from these distant points is hardly to be distinguished from the trap of the Connecticut Valley.

4. *Wood Tin in Georgia;* by WILLIAM P. BLAKE. (From a communication to one of the editors.)—In 1860, while examining a series of specimens of the residual black sand from the sluices used in collecting gold in North Carolina and Georgia, I found several minute grains of wood tin in the sand from the Nacoochee Valley, White County, Georgia. Although it occurs sparingly, the fact that it exists is worthy of record, as it may possibly be traced to larger deposits. I have examined sand from a great number of other localities, southwestward from Rutherfordton in North Carolina, without finding any traces of tin. The usual minerals of the "black sand" about Dahlonega, Georgia, are specular iron, magnetite, ilmenite, rutile, cyanite and garnet. At the Walton Branch, in North Carolina, corundum, zircon and monazite are abundant, with the ordinary mixtures of iron minerals, and xenotime occurs in minute crystals, but no tin ore was found.

5. *Das Erdbeben von Herzogenrath am 22 October*, 1873, *Ein Beitrag zur exakten Geologie;* A. VON LASAULX. Bonn, 1874. pp. 157, with several plates.—During the autumn of 1873 the

neighborhood of Aachen (Aix la Chapelle) was the center of a series of earthquake shocks, which continued from September 28th to December 2d; the most violent and extended of them, however, took place on the 22d of October. This earthquake has been the subject of minute and careful study by v. Lasaulx, and all directly or indirectly interested in such matters will find his pamphlet of great interest. After giving all the observations made at different places, he discusses the general character of the earthquake, that is, its intensity and extent, duration, and direction, with the accompany phenomena of sound, etc. He determines also, by careful calculations, the exact superficial center of the action, and also the center in the earth's interior from whence it went out, and its velocity of propagation (360 meters per second). His conclusion upon the second point is important and interesting, he says: the center of propagation (Ausgangspunkt der Erschütterung) did not lie at a depth so great that the direct cause of the first impulse could have been on the limiting zone between the fluid interior of the earth and the solid crust, nor could it have been in any immediate connection with this; on the contrary, this center must have been in the region of the older sedimentary rocks. He adds that it is not improbable that it was connected with the making of cracks and fissures in the earth's crust.

The seismometer, or seismochronograph, is a little instrument devised by the author for registering the time of earthquake shocks. It is intended to be attached to the reliable clocks to be found in telegraph offices and places of that kind, and acts by means of a little lever which falls, stopping the pendulum, at the moment of the shock. This is effected by means of a metal ball, which is dislodged from its delicate resting-place and sets free a spring, which in turn acts upon the lever. The direction of the shock is also approximately recorded. The author justly urges that, with some such apparatus in general use, our observations of the time of earthquake phenomena would be much more numerous and trustworthy. E. S. D.

6. *Mineralogische Mittheilungen, gesammelt von G. Tschermak.* Heft II, 1874. Vienna. 77 pp.—Prof. Tschermak is performing a great service to all mineralogists in collecting and making available the memoirs which fill the pages of the successive numbers of the Mittheilungen. His position as Director of the Royal Mineralogical Museum of Vienna—a city well known for its scientific activity—gives him especial advantages for obtaining valuable contributions. This journal appears quarterly in connection with the Jahrbuch der k. k. geol. Reichsanstalt, though it is also published independently. It is now in its fourth year, and as the only journal devoted exclusively to mineralogy, already holds a high place among scientific serials. The last number received is the second issued for 1874, and contains the following papers:

Simple crystals of Albite from the Schneeberg, by J. Rumpf; Morphological study on Atacamite, by Edward S. Dana (New

Haven); On the occurrence of native Iron in a dike of Basalt at Ovifak in Greenland, by G. Nauckhoff; Monograph on Roselite, by A. Schrauf; On Clinochlore, by A. Schrauf; On the occurrence of Meteorites at Ovifak, Greenland, by G. Tschermak; Analyses of Feldspar, Chinochlore (from Chester county, Penn.), Magnesia-mica, Mispickel crystals, etc., from the laboratory of Prof. Ludwig; also short notices of Glauberite from Sicily, of Stalagmites from the Adelsberg Grotto, and of a new and interesting twin (*drilling*) of Calcite.

7. *Description de la Formation Carbonifère de la Scanie*; par E. Erdmann.—Stockholm, 1873. 84 pp. 4to.—This is an abridged edition, in French, of Erdmann's Swedish memoir. The carboniferous formation described is supposed to be of the age of the Lias. Besides this and the Pre-Silurian formation, there are in Scania the Upper and Lower Silurian overlaid conformably by the sandstone of Hör, which Erdmann inclines to make the base of the Scanian Carboniferous. The Carboniferous has afforded plants and mollusks which render it probable that it is not older than the Lias. Above, there are Cretaceous beds, probably over 150 meters in thickness, and then the Quaternary.

8. *Petrographische Studien an den Phonolithgesteinen Bohemens;* Dr. E. Boricky. Prague, 1874. pp. 93, with two colored plates.—The contributions of Dr. Boricky upon the basaltic rocks of Bohemia have already been noticed in this Journal. This pamphlet upon the phonolytes is of the same character, and is marked by the same care and minuteness of description. The beautiful colored plates will be of great help to those studying the memoir.

9. *Descriptions of the Mollusks of the Tertiary of Piedmont and of Liguria;* by L. Bellardi.—Volume xxvii of the Memoirs of the Turin Academy, published in 1873, contains Part I. of this memoir. It occupies pages 33 to 294, and is illustrated by 15 beautiful and well crowded lithographic plates. The species include the Cephalopods, Pteropods, and Heteropods, and the Gasteropods of the families Muricidæ and Tritonidæ.

10. *Beskrifning öfver Besier-ecksteins kromolitografi och litotypografi använda vid tryckningen af geologisk Öfversigtskarta öfver Skåne, meddelad af* Algernon Börtzell.—No geological charts are more admirably colored than those of Sweden. This paper, giving an account of the method, and representing by a colored plate the mixtures of colors for producing 200 different shades of color, is of the highest interest to the geologists of this country, and all workers in chromolithography.

11. *Das Elbthalgebirge in Sachsen* von Dr. H. B. Geinitz; I Theil, 7 Lieferung.—This new number contains eight new lithographic plates, 53 to 60 inclusive, of Gasteropods from the Lower Quader or Middle Cretaceous, representing more than a hundred species.

12. *Geological Survey of Georgia.*—A survey of the State of Georgia is in progress under the direction of Professr Little, with Mersrs. McCutchen and Schley as assistants.

13. *Geological Survey of Sweden.*—The geological survey of Sweden is at present under O. M. Torell as chief geologist; the other geologists are A. E. Törnebohm, E. Erdmann, D. Hummel, O. J. Gumælius, M. J. Stolpe; assistant geologists, V. Karlsson, J. G. O. Linnarsson, L. J. Palmgren; actuary, J. E. Börtzell; chemist, G. H. Santesson.

14. *Memoirs of the Geological Survey of Italy.*—The second part of volume ii of these memoirs contains a paper by B. Gastaldi on the ophites or Pietri verdi of the Alps.

15. *American Entomology.*—A record of American Entomology for 1873, by A. S. PACKARD, Jr., is contained in the Sixth Annual Report of the Trustees of the Peabody Academy of Science at Salem, Mass.—The same report contains also descriptions of N. A. Noctiudæ by A. R. Grote, of N. A. Phalænidæ, by A. S. Packard, Jr., besides others of N. A. Phyllopoda by A. S. Packard, Jr., and notes on some dredgings near Salem by A. E. Verrill.

16. *The Carnivorous Habits of some of our Brother-Organisms —Plants*, is the theme chosen for his address by the president of the Department of Zoology and Botany (Dr. Hooker), at the Belfast meeting of the British Association. Although the subject owes the great interest it now inspires chiefly to some sagacious investigations by Mr. Darwin, still mainly unpublished, it has been so much more attended to in a popular way in the United States that it has no longer here the complete novelty which it appears to possess on the other side of the Atlantic. Here the principal facts, as known up to a recent date, along with those relating to plant-climbing and insect-fertilization, are matters of school instruction; and a narrative in The Nation recapitulated what is known, and what has been observed in this country especially, from the time of Macbride, and later, of Dr. Curtis, down to contributions of Mr. Canby and Mrs. Treat. Also the curious new discoveries made last summer by Dr. Mellichamp upon the most interesting of the Sarracenias, given in full in the New York Tribune, and in abstract in this Journal, were presented anew, and with further particulars, to the American Association, at the Hartford meeting early in August; and Mr. Canby supplemented these with an account of the behavior of the California analogue of *Sarracenia*, viz., *Darlingtonia*.

The report which has reached us of Dr. Hooker's interesting dissertation is unofficial and evidently somewhat imperfect, having many of the literary faults incident to a reporter's transcript. But its new points—of which alone we can now take notice—are clearly presented and are full of interest. Passing by the resuscitation of Linnæus' forgotten suggestion that the *Sarracenia* leaf may be a metamorphosis of that of *Nymphæa*, hollowed out into a cup to hold the water in which it can no longer float (on the strength of which he claims Linnæus as a Darwinian evolutionist!), and the classification of the pitchers of the several species according to their endowments and adaptations, which has been before alluded to; and interposing only the remark that in wild and even in

cultivated plants of *Sarracenia flava* we have found the watery secretion bedewing the inside of closed pitchers nearly ready to open, gathering into drops, and trickling toward the bottom of the cavity; we proceed to *Nepenthes*, the pitcher-plants of the oriental tropics and of hot houses, of which Dr. Hooker has made a special study. Time has not yet been secured for the completion of the investigations; but Dr. Hooker has made out the following capital points: 1. The rim of the Nepenthes pitchers in all the species examined "secretes honey," a lure to insects, etc.; so also does the under side of the lid when this remains overhanging, but not in those species with lid everted. In the latter, honey on the lid would tend to allure insects away from the pitcher instead of into it. 2. Below the rim is a very smooth, opaque surface, affording no foothold to insects, and producing no secretion. 3. "Below this zone the interior of the pitcher is entirely occupied by the secretive surface, consisting of a cellular floor crowded with spherical glands in inconceivable numbers. Each gland precisely resembles a honey-gland of the lid, and is contained in a pocket of the same nature, but semicircular, with the mouth downward, so that the secreted fluid all falls to the bottom of the pitcher. In *Nepenthes Rafflesiana* 3,000 of the glands occur on a square inch of surface, and upward of a million in an ordinary-sized pitcher. I have ascertained that, as was indeed to be expected, they secrete the fluid which is contained in the bottom of the pitcher before this opens, and that the fluid is always acid." 4. A further secretion of this fluid is excited by supplying the pitcher with animal matter. "When the fluid is emptied out of a fully formed pitcher that has not received any animal matter, it forms again, but in comparatively very small quantity. . . . . I do not find that placing inorganic substances in the fluid causes an increased secretion; but I have twice observed a considerable increase in pitchers after putting animal matter in the fluid." 5. This fluid evidently digests animal matters. Treated with "white of egg, raw meat, fibrine, and cartilage," in all cases the action is most evident, in some surprising. After twenty-four hours' immersion, the edges of cubes of white of egg are eaten away and the surfaces gelatinized. Fragments of meat are rapidly reduced, and pieces of fibrine weighing several grains dissolve and totally disappear in two or three days. With cartilage the action is most remarkable of all; lumps of this weighing eight and ten grains are half gelatinized in twenty-four hours, and in three days the mass is greatly diminished and reduced to a clear, transparent jelly." "Little action takes place in any of the substances placed in the fluid drawn from pitchers and placed in glass tubes, nor has any followed after six days' immersion of cartilage or fibrine in pitchers of *N. ampullaria* placed in a cold room, whilst on transferring the cartilage from the pitcher of *N. ampullaria* in the cold room to one of *N. Rafflesiana* in the store, it was immediately acted upon." From this it is conjectured that something analogous to

pepsine is secreted by the pitcher after the reception of the animal matter.

In speculating, at the close, upon this apparently wholly anomalous inversion of the functions of plants and animals, Dr. Hooker compares the phenomena he has been considering with the nourishment which the embryo of albuminous seeds, when germinating, draws from the deposit which surrounds or is in contact with it, referring also to Van Tieghem's experiments, in which an artificial emulsion was successfully substituted for the real endosperm. The analogy is hardly good; all plants and all organs use organized matter in their formation and are wholly made of it; the growth of an embryo, a bud or shoot, and of a cell, are alike in this. The difference is that they feed upon vegetable, not upon animal matter; upon matter assimilated by the plant itself, not upon matter further assimilated by an animal. Nor is the real analogy quite reached in the comparison with parasitic plants, even "flowering plants that pass through their lives without ever doing a stroke of the work that green plants do." It is found, however, in parasitic plants of a lower grade which feed directly upon animals or animal matter, living or dead;—from which point of view these higher carnivorous plants under consideration may not only "find their place as one more link in the continuity of nature," and be thought to have done so through gradations such as Dr. Hooker suggests, but they may also be conceived as cases of far-reaching *atavism*.

As these subjects become matters of popular interest, the grossest misapprehensions and mis-statements must be expected. In England, the Graphic leads off with a well executed page of woodcuts, swarming with flies and hornets, some of which are busy about the mouth of a nondescript *Sarracenia*, having a curiously lobed or scolloped lid. The letter-press describes *Nepenthes* and *Cephalotus* as having "lids which shut down upon their victims," while *Darlingtonia* "curls its leaf around them," and so on.

A. G.

17. *Linnean Society of London.*—Upon the retirement of Mr. Bentham, after twelve or thirteen years of most efficient service, Prof. Allmann, of Edinburgh, was elected president at the last anniversary meeting, the zoologists taking their regular turn. The choice is a happy one. It is to be hoped that the new president will continue the annual addresses which his predecessor initiated and made so interesting The society is established in the commodious quarters provided for it by Government in the new portion of Burlington House, the apartments of late temporarily occupied being given up to the Royal Society. One of the Fellows is now appointed to take charge of the publication of the Journal of the Linnean Society, the editorial care of which had mainly devolved upon the late president. Some changes in the by-laws which seem to have been required in order to secure responsible editorship, and which were adopted upon the recommendation of the council, were yet contested in a manner which

caused the society to take legal advice upon the matter. The award of Lord Hatherley, now published, affirms the validity of the new enactment, and so settles, it is to be hoped finally and satisfactorily, a controversy which, being wholly of a domestic nature, had better not have been referred to in the scientific journals. A. G.

18. *Restored Professorship of Botany at the Jardin des Plantes, Paris.*—One of the three chairs of Botany at the *Jardin des Plantes*, namely, the one long occupied by the Jussieus, was suppressed after the death of *Adrien de Jussieu*, in 1853, and a chair of paleontology established instead. Thanks to the exertions of Count Jaubert, this botanical chair has been reconstituted, and M. Bureau has been named to fill it. M. Maxime Cornu succeeds M. Bureau as *aide-naturaliste*. The reorganized laboratory of instruction at the Garden, under the charge of these two active botanists, has been in most successful operation during the past season. A. G.

III. Astronomy.

1. *On the Spectrum of Coggia's Comet;* by Dr. Huggins.—The new point noticed in this communication was that the bands of the comet were so far shifted as to indicate—supposing there really was carbon in the comet—that the relative motion of the approach of the comet to the earth was forty-six miles per second. The comet really, however, approached the earth at the rate of twenty-four miles per second; and it was therefore uncertain whether the whole or part of the difference in this velocity was due to the motion of matter within the comet. The brighter portion of the head of the comet was due evidently to a larger proportion of the matter giving a continuous spectrum. It seemed probable, therefore' to the author that the nucleus was solid, heated by the sun and throwing out matter which formed the coma and tail; and part of this was in a gaseous form, giving the spectra of bright lines. The other portion existed probably in small incandescent particles; the polariscope showing that certainly not more than one-fifth of the whole light was reflected solar light.—*Proc. Brit. Assoc., Nature*, Sept. 10.

2. *Meteoric Iron of Iquique, in Peru;* by Gustaf Rose.—This paper was left completed but unpublished by its eminent author, and has now been included in the volume issued by the "Gesellschaft Naturforschender Freunde der Berlin" as the "Festschrift" of its recent Centennial meeting. This mass of meteoric iron originally weighed twenty-five pounds. The part in the museum of the University weighs twenty pounds. It was found ten leagues to the east of the village of Iquique near the western boundary of the Pampa of Tamarugul. Raimondi, the original describer of it, obtained for the specific gravity 7·86. In an analysis by Rammelsberg, 2·66 per cent was removed by a solution of chloride of mercury. The composition of this part gave—

Iron	Nickel	Phosphorus	Insol. in H^2Cl	
2·17	0·37	0·05	0·07	=2·66

The nickel and cobalt of the insoluble part was found to be 15·49 nickel and 0·19 cobalt. This left 81·66 for the iron. Raimondi found three pieces of it to contain 81·42, 85·61, 87·59 of iron, and 18·52, 14·37, 12·38 of nickel.

3. *Meteorites.*—A meteorite which fell near Virba (?) in Turkey on the 20th of May last has been described before the Academy of Sciences of Paris by Daubrée. It has a grayish silicated exterior and contains numerous grains of nickeliferous iron and sulphide of iron. It contains also some grains of chromic iron.

Daubrée states also that he had obtained four new fragments of the meteorite which fell at Saint Amand (Loir-et-Cher) in 1872. —*L'Institut*, Aug. 5.

IV. Miscellaneous Scientific Intelligence.

1. *American Meteorology.*—Mr. L. Blodget, in a paper published in the Proceedings of the American Philosophical Society, vol. xiv, at p. 150, after alluding to the fact that an easterly movement in the general circulation of the wind has been made certain by observations on Pike's Peak and Mt. Washington, proceeds to mention facts that establish another point in meteorology: —that the extremes of cold observed at times, especially over the northwestern interior of the United States, were not propagated along the surface, but were produced where they occur, "as if brought down from the upper atmosphere, or as if the result of the action of causes extraneous to the earth's atmosphere." He concludes that the greater number of winds in cold weather particularly are winds that descend, and that to this descent most of their continued force is due. In the course of his paper he says:

"The descent of masses of heavy, cold air, must often be induced simply to fill the void caused by contraction of the volume of air from which rain and snow fall. All along the belt of westerly winds this contraction is going on, and this very rapidly during all the colder months. Moving with a constant motion toward the earth, as well as along the surface, it is only a natural vicissitude of this condition, that the descending mass should, at intervals, be poured, like a mass of cold water, over the border of the humid belt, producing the extremes that so often appear to *strike down from above.*" * * *

"I venture to assume, therefore, a large measure of influence in causing extremes of cold in these latitudes to the descending volume of air incident to the shrinking and wasting of heat and moisture from the atmospheric current eastward in the course of traversing the continent. Its northern border is perpetually invaded by fitful alternations of displacement; sometimes getting calm and intensely cold, reducing the temperature in winter to 10°, 20° or 30° F. below zero; and in spring, when the general

accession of heat gives a more free play of the forces, a frequent recurrence of heavy northwest dry winds *poured from above*, and from the north, displacing and condensing the local or surface atmosphere; and this overflow is almost constantly repeated until the whole system of circulation has been swept beyond our limits at the north, by the advance of summer. During most of the summer months the rarifying and expanding forces prevail so completely, as to remove all these phenomena far to the north, or possibly to another hemisphere."

2. *Preliminary Map of Central Colorado*, showing the region surveyed in 1873; primary triangulation by J. T. GARDNER; topography by G. R. BECHLER, H. GANNETT and A. D. WILSON. U. S. Geol. and Geogr. Survey of the Territories, F. V. HAYDEN, U. S. Geologist in charge. Department of the Interior.—This map, with the accompanying sketch showing the triangulation, contains, besides the positions of rivers and settlements, the locations of a very large number of heights, the elevation above the sea level of many of them, the lines used in the primary and secondary triangulation and the positions that were occupied for the survey.

The mountain ranges include the following: (1.) The *Eastern* or *Front* Range, in which stands Long's Peak, 14,271 feet high, and Pike's Peak, 14,147 feet, about ninety miles apart. West of this, (2.) the *Park* Range, east of the head waters of the Arkansas, containing Mt. Powell, 13,398 feet high, Mt. Lincoln, 14,296 feet, and several other summits over 13,000 feet in height. (3.) Next, west of the Arkansas, the *Sawatch* Range, in which, beginning at the north, there are the Mountain of the Holy Cross, north of the head waters of the Arkansas, near latitude 39½° and the meridian of 106°, 14,176 feet high, Massive Mt. 14,368 feet, Mt. Elbert 14,326 feet, La Plata Mt. 14,302 feet, Grizzly Peak 13,315 feet, Mt. Harvard, 14,383 feet, Mt. Yale, 14,151 feet, Mt. Princeton, 14,199 feet, Mt. Antoro, 14,245 feet, Mt. Shavano 14,093 feet, Mt. Ouray, 14,043 feet, the last near the parallel of 38°; then the continuation south southwestward in the Sangre de Christo Range, in which are Hunt's Peak, 12,446 feet high, Mt. Rito Alto, 12,989 feet, Mt. Crestoner, 14,233 feet, the last near the parallel of 38° on the meridian of 105½°. (4.) Still farther west, crossing the meridian of 107° near the parallel of 39°, the *Elk* Mountains, containing, beginning at the north in latitude 39° 15′, and longitude 107° 10′, Sopris Peak, 12,972 feet high, Capital Mountain, 13,992 feet, Snow Mass Mt. 13,961 feet, Maroon Mt. 14,000, Gothic Mt., on the meridian of 107°, 12,491 feet, Castle Peak, more to the east on the parallel of 39°, 14,106 feet, White Rock, 13,847, Crested Butte, 12,014 feet, Italia Peak, farther east, 13,491 feet. These Elk Mountains lie at the head of the San Juan and Grand Rivers, tributaries of the Colorado. The map registers the results of a great amount of topographical work for a single season.

3. *Tableau des Terrains Sedimentaires;* par E. RENEVIER, Professor of Geology in the Academy of Lausanne; 36 pp., with a series of nine tables in as many folded sheets. 1874 (Bull. Soc.

Vaud. Sci. Nat., Nos. 70, 71, 72).—The tables, which are the chief part of this important memoir, and must have cost the author much labor, are arranged in columns, and present the synonymy and equivalency of the various subdivisions of the periods in geology, with also the prominent localities in different countries, and the characteristic fossils. They commence with the Quaternary, to which one sheet, of a pale yellow color, is devoted. The next sheet, of an ochre-yellow color, includes the Pliocene and Miocene, under the head of the Neogen or Molassic period, for each of the seven subdivisions of which the names of a score or more of fossils are given, and in other columns, the localities and synonymy of different countries. The third, of a bright yellow, comprises the "Eocene or Nummulitic Period." The fourth, of a green color, the Cretaceous, under which fourteen subdivisions or stages are given; and so on through the series to the first of the Paleozoic. The tables will be found very convenient as a help toward understanding the names of subdivisions used in Europe, and also in many other ways.

4. *Franz-Joseph Land.*—The land discovered by the Austrian Polar expedition under Lieut. Weyprecht lies to the north of Nova Zembla over the meridians 58° and 59° and between the parallels of 80° 15′ and 83°, the latter being the most northerly point observed. It is stated to be about as large as Spitzbergen, with many fiords and numerous islands off the coast. It is mountainous; the elevation of the ridges averages 2,000 or 3,000 feet, and the highest summit to the south, named Mt. Humboldt, is 5,000 feet above the sea. Glaciers were of great extent. The ridges are dolomitic. Elk, hares, and traces of foxes and bears were found, and myriads of birds.

5. *Observaciones Magneticas y Meteorologicas del Colegio de Belen de la Compania de Jesus, en la Habana. Año Meteorologico de* 1872. Small folio. Habana, 1874.—The Meteorological Observatory of the college of Belen is under the charge of Señor Benito Viñes, S. J. The observations recorded have evidently been made with great care and fullness. They are presented in tables for each month, with diagrams showing the changes for the month under the heads: velocity of the wind, direction of the wind, horizontal force, declinometer, hygrometric state, tension of the vapor of water, thermometer and barometer.

6. *Table for Dilution of Alcohol.*—The following table, constructed by Berguier, gives the quantity of distilled water necessary to reduce alcohol of a certain known percentage to any desired lower degree of strength. Opposite the number representing the percentage of alcohol in the given sample are placed the quantities of it, and of water, respectively, necessary to produce alcohol of the percentage indicated at the top of any column. Thus, to obtain alcohol for 80° from that of 94°, the number 94 is sought for in the first column, and opposite to it, under the column "Required Strength," "80°," the numbers 808 and 192 indicate respectively the quantities, by weight, of alcohol of 94°, and

of distilled water necessary to produce 1,000 parts of alcohol at 80°. For the sake of convenience, the specific gravities of each grade are given in a separate column.

Table of the relative quantities, by weight, of alcohol of different degrees of concentration, and of distilled water necessary to produce 1,000 *parts of alcohol of a desired lower degree of concentration.*

Strength, in per cent, of alcohol employed.	Specific Gravity.	Required Strength.									
		90° 0·8228 sp.gr.		85° 0·8357 sp.gr		80° 0·8483 sp.gr		60° 0·8956 sp.gr		56° 0·9047 sp.gr	
		Alcoh.	Water	Alcoh.	Water	Alcoh.	Water	Alcoh.	Water	Alcoh.	Water
100°	0·7938	857	143	795	205	735	265	522	478	482	518
99	0·7969	871	129	807	193	747	253	530	470	490	510
98	0·8001	885	115	820	180	759	241	539	461	498	502
97	0·8031	899	101	833	167	771	229	547	453	506	494
96	0·8061	913	87	846	154	783	217	555	445	514	486
95	0·8089	927	73	859	141	796	204	564	436	522	478
94	0·8118	942	58	873	127	808	192	573	427	530	470
93	0·8145	956	44	886	114	820	180	582	418	538	462
92	0·8172	970	30	899	101	832	168	590	410	546	454
91	0·8199	985	15	913	87	845	155	599	401	554	446
90	0·8228	---	---	927	73	858	142	609	391	563	437
89	0·8254	---	---	941	59	871	129	618	382	571	429
88	0·8279	---	---	955	45	884	116	627	373	580	420
87	0·8305	---	---	970	30	898	102	637	363	589	411
86	0·8331	---	---	985	15	912	88	646	354	598	402
85	0·8357	---	---	---	---	926	74	656	344	607	393
84	0·8382	---	---	---	---	940	60	667	333	616	384
83	0·8408	---	---	---	---	955	45	677	323	626	374
82	0·8434	---	---	---	---	969	31	687	313	636	364
81	0·8459	---	---	---	---	994	16	698	302	646	354
80	0·8483	---	---	---	---	---	---	709	291	656	344
79	0·8508	---	---	---	---	---	---	720	280	666	334
78	0·8533	---	---	---	---	---	---	732	268	677	323
77	0·8557	---	---	---	---	---	---	744	256	688	312
76	0·8581	---	---	---	---	---	---	756	244	699	301
75	0·8603	---	---	---	---	---	---	768	232	710	290
74	0·8625	---	---	---	---	---	---	781	219	722	278
73	0·8649	---	---	---	---	---	---	794	206	734	266
72	0·8672	---	---	---	---	---	---	807	193	747	253
71	0·8696	---	---	---	---	---	---	821	179	759	241
70	0·8721	---	---	---	---	---	---	835	165	772	228
69	0·8745	---	---	---	---	---	---	849	151	785	215
68	0·8769	---	---	---	---	---	---	864	136	799	201
67	0·8793	---	---	---	---	---	---	880	120	813	187
66	0·8816	---	---	---	---	---	---	896	104	828	172
65	0·8840	---	---	---	---	---	---	911	89	843	157
64	0·8863	---	---	---	---	---	---	928	72	858	142
63	0·8886	---	---	---	---	---	---	946	54	874	126
62	0·8908	---	---	---	---	---	---	963	37	891	109
61	0·8932	---	---	---	---	---	---	981	19	907	93
60	0·8956	---	---	---	---	---	---	---	---	925	75
59	0·8979	---	---	---	---	---	---	---	---	943	57
58	0·9001	---	---	---	---	---	---	---	---	961	39
57	0·9025	---	---	---	---	---	---	---	---	980	20

—14 C. ccxii, 1874, 251.

7. *Tin-bearing country, New England, in New South Wales, Australia.*—A report by the Licensed Surveyor, C. S. Wilkinson, to the Surveyor General, dated July 14, 1873, contains the following among its statements. The region described lies within a radius of about twenty-five miles from Inversell to the south and east. The principal tin mines are on Cope's Creek, Middle Creek and Macintyre River. The rocks are granites, greenstone trap, Carboniferous beds, Miocene, Pliocene and Quaternary.

The Quaternary includes drift deposits. On the Macintyre valley the stratified drift is in terraces of various heights above the river. Rev. W. B. Clarke states that he has traced some of this drift at great heights above the valley for more than eighty miles. They are evidently the work of the waters from the melting glacier and those of the continued floods during the time of high latitude depression of (in the northern hemisphere, at least) the Champlain period.

The Pliocene of the region includes extensive basaltic outflows. The Tertiary abounds in stems and leaves of plants of the genera *Laurus*, *Cinnamomum*, *Daphnogene*, and others, which are referred to the Lower Miocene, M'Coy finding some species closely like those of Oeningen and of the vicinity of Bonn.

The Carboniferous beds are of the same age with those of the Hunter.

Stream tin is found in the Drift, and also in the Miocene; and valuable veins of tin ore occur in the granite. The granite is stated to be closely like that of Cornwall, and is pronounced of Upper Carboniferous age.

The amount of tin ore raised during 1872 was about 800 tons. The tin-mining region of Inversell is but a small portion of the stanniferous country of New South Wales and Queensland.

8. *Tortoises of Mauritius closely related to those of the Galapagos, places that are nearly antipodes to one another.*—Dr. A. Günther, in a memoir on "the Living and Extinct Races of Gigantic Land-Tortoises," Parts I and II of which have been published, states that there are remains of gigantic Tortoises on Mauritius and the neighboring island of Rodriguez associated with those of the Dodo and Solitaire, which indicate that the races have only recently become extinct. They differ from other Tortoises of the region in having a flat cranium and truncated beak, and in this respect they have the greatest affinity with the tortoises still inhabiting the Galapagos Archipelago. Dr. Günther observes that the presence of these allied tortoises at points so remote from one another can be accounted for only on the view that *they are in each case indigenous.*—*Ann. Mag. Nat. Hist.*, xiv, 311, Oct., 1874.

9. *The Journal of the Franklin Institute*, Philadelphia.—The August number of this excellent journal contains a paper by Prof. S. P. Langley, on the external aspects of the Sun,—its photosphere and spots, its chromosphere and corona, which is a valuable review of this whole subject, as viewed by an astronomer who

had himself made original observations on the subject. It is illustrated by a plate of one of the sun-spots, which is as wonderfully well executed as the spot is marvelous in itself.

This scientific monthly, devoted to physics and practical chemtry, as well as mechanical science, is now edited by Prof. George F. Barker (Professor of Physics in the University of Pennsylvania), and could not be in better hands.

10. *Transactions of the Wisconsin Academy of Sciences, Arts, and Letters, Madison, Wisconsin;* vol. ii, 1873. 254 pp., 8vo.—Among the papers in this volume are the following: R. Irving, on some points in the geology of northern Wisconsin; P. R. Hoy, on some peculiarities in the fauna near Racine; W. W. Daniels, on the rapidity of the absorption of arsenic by the human liver; T. C. Chamberlin, on some evidences bearing upon the method of the upheaval of the quartzites of Sauk and Columbia Counties, and on fluctuations in the level of the same quartzites; R. Irving, on the junction of the Primordial sandstones and Huronian schists in Wisconsin, and on the occurrence of gold and silver in quartz from Clark County.

Dr. J. W. Hoyt, of Madison, is president of the Academy.

11. *Cincinnati Quarterly Journal of Science.*—The number for October, the last of the first volume, contains descriptions of new species of mollusks from the Cincinnati group by the Editor J. A. Miller; a paper on other species from the same group by U. P. James; also descriptions of new species of *Lichenocrinus*, *Glyptocrinus* and *Beyrichia*, besides other papers of interest.

OBITUARY.

M. Elie de Beaumont.—On the 24th of September died Elie de Beaumont, the eminent geologist, and long the "Secrétaire Perpetuel" of the Academy of Sciences of Paris. Born on the 25th of September, 1795, he entered the Polytechnic School in 1819, and leaving it with the highest honors, entered the Ecole des Mines in 1821. He became Professor in the Ecole des Mines in 1829, Professor in the Collége de France in 1832, Engineer in Chief of Mines in 1833, Inspector-General of Mines and Member of the Academy of Sciences in 1835, its Perpetual Secretary in 1853 in place of Arago, Senator in 1854, and Grand Officer of the Legion of Honor in 1860.

Dr. Friedrich Hessenberg died in his native city of Frankfort, on the 8th of last July. A jeweler by trade, he yet found time to devote to his favorite science of mineralogy, and though entirely self-educated, he ranked among the foremost mineralogists and crystallographers in Germany. His "Mineralogische Notizen," published in 11 numbers, contain some of the most important contributions ever made to the science.

Half-hour Recreations in Popular Science, Dana Estes, Editor, No. 12. The Circulation of the Waters on the Surface of the Earth, by H. W. Dove. Boston, 1874.

Fig. 1.

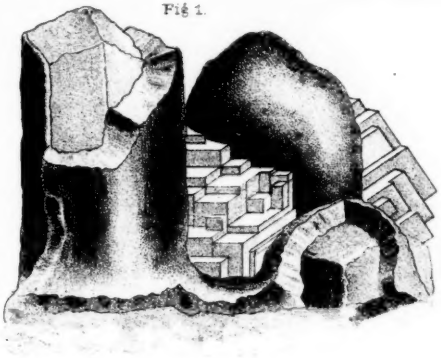


Fig. 2.

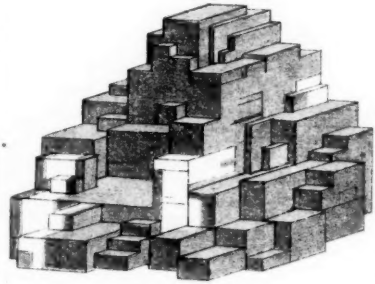


Fig. 4.

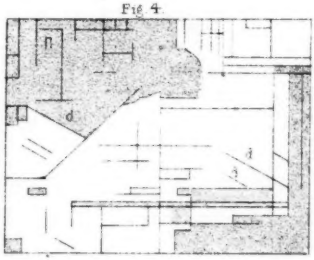


Fig. 3.

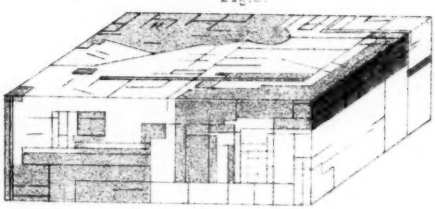
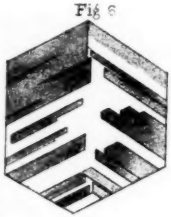


Fig. 5.

Fig. 6



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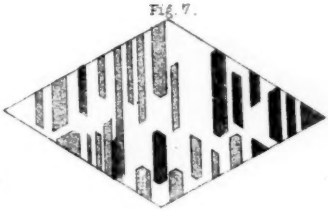

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